AUGUSTINE TO
GALILEO

VOL II

SCIENCE IN THE LATER MIDDLE AGES
AND EARLY MODERN TIMES
XIII–XVII CENTURIES

A. C. Crombie
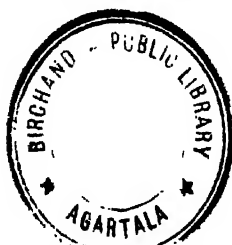
MERCURY BOOKS
LONDON
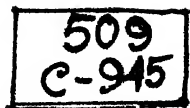
TO NANCY

ACKNOWLEDGEMENTS (VOL. II)

CONTENTS

Volume I, SCIENCE IN THE MIDDLE AGES: V–XIII
CENTURIES, consists of the following chapters:

PLATES

Following page 180

AUGUSTINE TO GALILEO

VOL II

I

SCIENTIFIC METHOD
AND DEVELOPMENTS
IN PHYSICS IN THE LATER
MIDDLE AGES

(1) THE SCIENTIFIC METHOD
OF THE LATER SCHOLASTICS

The activity of mind and hand that showed itself in the additions of scientific fact and in the development of technology made in the 13th and 14th centuries is to be seen also in the purely theoretical criticism of Aristotle's theory of science and fundamental principles that took place at the same time. This criticism was to lead later to the overthrow of the whole system of Aristotelian physics. Much of it developed from within Aristotle's scientific thought itself. Indeed Aristotle can be seen as a sort of tragic hero striding through medieval science. From Grosseteste to Galileo he occupied the centre of the stage, seducing men's minds by the magical promise of his concepts, exciting their passions and dividing their allegiances. In the end he forced them to turn against him as the real consequences of his undertaking gradually became clear; and yet, from the depths of his own system, he provided many of the weapons with which he was attacked.

The most important of these weapons were made by new ideas on scientific method, especially by new ideas on induction and experiment and on the role of mathematics

in explaining physical phenomena. These gradually led to an entirely different conception of the kind of question that should be asked in natural science, the kind of question, in fact, to which the experimental and mathematical methods could give an answer. The field in which the new kind of question was to produce its greatest effects from the middle of the 16th century was in dynamics, and it was precisely Aristotle's ideas on space and motion that came in for the most radical criticism during the later Middle Ages. The effect of this scholastic criticism was to undermine the foundations of his whole system of physics (with the exception of biology) and so to clear the way for the new system constructed by the experimental and mathematical methods. At the end of the medieval period a fresh impetus was given to mathematics and mathematical physics by the translation into Latin and printing of some previously unknown or little known Greek texts.

It must always be remembered when reading medieval scientific writings that these were composed, just as a modern scientific paper is composed, within the context of an accepted manner of discussion and of a given nexus of problems. The academic context of discussions of logic and method and of mathematics and natural science was primarily the arts course, and further scope in certain branches of science was provided for those who went on to study medicine. The normal manner of discussion was in the form of the commentary, which by the fourteenth century had developed into the method of proposing and discussing specific problems or *quæstiones* (see Vol. I, pp. 13, 147, 180–82, 223). A modern reader may be puzzled by a commentary or treatise that takes up the discussion of a problem in the middle and assumes not only a knowledge of the background but also the appropriateness of the manner and methods of proposing a solution. Certainly medieval scientific writings are not always self-explanatory or easy to read. Many of them almost seem to be specially designed to mislead the 20th-century reader. We will be certainly misled if we fail to realise that the commentary was not simply an exposition of the text of Aristotle or some other 'authority' but that it, and even more the

quæstiones, were the manner of offering criticisms and proposing original results and solutions. And we will be equally misled if we translate the more modern-sounding of those original solutions into 20th-century terms, and overlook the context of assumptions and conceptions in which they were proposed and the actual questions to which they were offered as answers. The fact that so many questions in medieval (and ancient) science overlap with similar questions in the context of modern science may be the greatest obstacle to historical understanding.

The great idea recovered during the 12th century, which made possible the immediate expansion of science from that time, was the idea of rational explanation as in formal or geometrical demonstration; that is, the idea that a particular fact was explained when it could be deduced from a more general principle. This had come through the gradual recovery of Aristotle's logic and of Greek and Arabic mathematics. The idea of mathematical demonstration had, in fact, been the great discovery of the Greeks in the history of science, and it was the basis not only of their considerable contributions to mathematics itself and to physical sciences like astronomy and geometrical optics, but also of much of their biology and medicine. Their bent of mind was to conceive of science, where possible, as a matter of deductions from indemonstrable first principles.

In the 12th century, this notion of rational explanation developed first among logicians and philosophers not primarily concerned with natural science at all but engaged in grasping and expounding the principles, first, of the *logica vetus* or 'old logic' based on Boethius and, later in the century, of Aristotle's *Posterior Analytics* and various works of Galen. What these logicians did was to make use of the distinction, ultimately deriving from Aristotle, between experiential knowledge of a fact and rational knowledge of the reason for, or cause of, the fact; they meant by the latter knowledge of some prior and more general principle from which they could deduce the fact. The development of this form of rationalism was, in fact, part of a general intellectual movement in the 12th century, and not

only scientific writers such as Adelard of Bath and Hugh of St. Victor, but also theologians such as Anselm, Richard of St. Victor and Abelard tried to arrange their subject-matter according to this mathematical-deductive method. Mathematics was for these 12th-century philosophers the model rational science and, like good disciples of St. Augustine and Plato, they held that the senses were deceitful and reason alone could give truth.

Though mathematics was regarded in the 12th century as the model science, it was not until the beginning of the 13th century that Western mathematics became worthy of this reputation. The practical mathematics kept alive in Benedictine monasteries during the early Middle Ages, and taught in the cathedral and monastery schools founded by Charlemagne at the end of the 8th century, was very elementary and limited to what was necessary to keep accounts, calculate the date of Easter, and measure land for the purposes of surveying. At the end of the 10th century Gerbert had initiated a revival of interest in mathematics, as he did also in logic, by collecting Boethius's treatises on those subjects. Although Boethius's treatise on arithmetic contained an elementary idea of the treatment of theoretical problems based on the properties of numbers, the so-called 'Geometry of Boethius' was, in fact, a later compilation from which most of his own contribution had dropped out. It contained certain of Euclid's axioms, definitions and conclusions but consisted mainly of a description of the abacus, the device generally used for calculations, and of practical surveying methods and the like. The writings of Cassiodorus and Isidore of Seville, the other sources of the mathematical knowledge of the time, contained nothing fresh (Vol. I, pp. 11–13).

Gerbert himself wrote a treatise on the abacus and even improved the current type by introducing apices, and a few other additions were made to practical mathematics during the 11th and 12th centuries, but until the end of the 12th century Western mathematics remained almost entirely a practical science. Eleventh- and 12th-century mathematicians were able to use the conclusions of the Greek geometers for practical purposes, but were unable to

demonstrate those conclusions, even though the theorems of the first book of Euclid's *Elements* became known during the 11th century and the whole of that work was translated by Adelard of Bath early in the 12th. Examples of 11th-century geometry are Francon of Liége's attempt to square the circle by cutting up pieces of parchment, and the correspondence between Raimbaud of Cologne and Radolf of Liége in which each vainly tried to outdo the other in an unsuccessful attempt to demonstrate that the sum of the angles of a triangle equals two right angles. Little better work was done till the end of the 12th century.

In arithmetic, the situation was somewhat better owing to the preservation of Boethius's treatise on the subject. For instance, Francon himself was able to show that it was impossible to express rationally the square root of a number not a perfect square. The marked improvements that took place in Western mathematics early in the 13th century occurred first in the fields of arithmetic and algebra, and this was due largely to the development of this earlier tradition by two scholars of originality. The first was Leonardo Fibonacci of Pisa, who had given the earliest complete Latin account of the Arabic, or Hindu, system of numerals in his *Libe· Abaci* in 1202 (see Vol. I, p. 51). In later works he made some highly original contributions to theoretical algebra and geometry, his basic knowledge being derived primarily from Arabic sources, but also from Euclid, Archimedes, Hero of Alexandria and the 3rd-century A.D. Diophantus, the greatest of the Greek algebraists. Fibonacci on some occasions replaced numbers by letters in order to generalise his proof. He developed indeterminate analysis and the sequence of numbers such that each is equal to the sum of the two preceding (now called 'Fibonacci sequences'), gave an interpretation of a negative solution as a debt, used algebra to solve geometrical problems (a striking innovation), and gave solutions of various problems involving quartic equations.

The second mathematician of originality in the 13th century was Jordanus Nemorarius, who shows no trace of Arabic influence but developed the Greco-Roman arith-

metical tradition of Nicomachus and Boethius, in particular the theory of numbers. Jordanus habitually made use of letters for the sake of generality in arithmetical problems and he developed certain algebraic problems leading to linear and quadratic equations. He was also an original geometer. His treatises contained discussions of old problems, such as the determination of the centre of gravity of a triangle, and also the first general demonstration of the fundamental property of stereographic projection, that circles are projected as circles (cf. Vol. I, pp. 115–20).

After Jordanus there was a gradual improvement in Western geometry as well as in other parts of mathematics. A number of important original ideas were added. In an edition of Euclid's *Elements*, which he produced in about 1254 and which remained a standard text-book until the 16th century, Campanus of Novara included a study of 'continuous quantities,' to which he was led by considering the angle of contingence between a curve and its tangent smaller than any angle between two straight lines. By using a mathematical induction ending in a *reductio ad absurdum*, he also proved the irrationality of the 'golden section' or 'golden number,' that is the division of a straight line so that the proportion of the smaller section to the larger equals that of the larger to the whole. He also calculated the sum of the angles of a stellated pentagon. In the 14th century the grasp of the principle of geometrical proof made possible the improvements introduced into trigonometry by John Maudith, Richard of Wallingford and Levi ben Gerson (see Vol. I, pp. 96–97), and into the theory of proportions by Thomas Bradwardine and his followers in Merton College, Oxford, and by Albert of Saxony and others in Paris and Vienna. This work on proportions, like the striking work of Nicole Oresme on the use of co-ordinates and the use of graphs to represent the form of a function, was developed chiefly in connection with certain physical problems; it will be considered on a later page. Of considerable importance also were the improvements introduced into the methods of calculation in the Hindu system of numerals during the 13th and 14th centuries. The methods of multiplying and dividing used by

the Hindus and Moslems had been very uncertain. The modern method of multiplication was introduced from Florence, and the modern technique of division was also invented during the later Middle Ages. This made division into an ordinary matter for the counting house, whereas it had formerly been a formidably difficult operation even for skilled mathematicians. The Italians also invented the system of double-entry book keeping, and the commercial nature of their interests is shown by their arithmetic books in which problems are concerned with such practical questions as partnership, exchange, simple and compound interest, and discount.

The recovery of the idea of a demonstrative science in which a fact was explained when it could be deduced from a prior and more general principle, and the great improvements in mathematical technique that took place in Western Christendom during the 13th century, were the chief intellectual achievements that made 13th-century science possible at all. But the medieval natural philosophers did not stop there in their thinking on scientific method. The new knowledge in fact raised important methodological problems, as general problems of scientific thinking. Specially important were the problems how, in natural science, to arrive at the prior principles or general theory from which the demonstration or explanation of particular facts was to proceed; and how, among several possible theories, to distinguish between the false and the true, the defective and the complete, the unacceptable and the acceptable. In their study of these problems the medieval philosophers investigated the logical relationship between facts and theories, or data and explanations, the processes of the acquisition of scientific knowledge, the use of inductive and experimental analysis to break down a complex phenomenon into its component elements, the character of the verification and falsification of hypotheses, and the nature of causation. They began to form the conception of natural science as in principle inductive and experimental as well as mathematical, and they began to develop the logical procedures of experimental inquiry,

which chiefly characterise the difference between modern and ancient science.

In classical antiquity several quite different conceptions of scientific method had been formed within the general scheme of demonstrative science. The postulational method expounded by Euclid became most effective in application to the highly abstract subjects of pure mathematics and of mathematical astronomy, statics and optics. At its purest it was not experimental: long chains of deduction followed from premisses accepted as self-evident. For example, most of the problems investigated by Archimedes, its greatest Greek exponent, required, even in mathematical physics, no actual experiments: in formulating the law of the balance and lever Archimedes appealed not to experiment but to symmetry. But in more complicated subjects, especially in astronomy, the postulated hypotheses had to be tested by checking quantitative conclusions deduced from them against observation.

Related to this form of argument was the dialectical method of Plato, in which the argument was conducted by provisionally accepting a proposition and then proceeding to show either that it led to a self-contradiction or a contradiction with something accepted as true, or that it did not. This gave grounds for rejecting or accepting it. The mathematical equivalent of this form of argument is the *reductio ad absurdum* widely used by Greek mathematicians.

When attempting to deal, not simply with abstract mathematical subjects, but with the more difficult problems of matter (living and dead), many Greek physicists again adopted a form of the postulational method, proposing theoretical, unobservable, particles out of which to construct a theoretical world to match the world that is observed. The outstanding example of this is Democritus' theory of atoms and the void; another is the physics of Plato's *Timæus* (see Vol. I, pp. 30–31, and below pp. 36–37).

In contrast with this abstractly theoretical approach stands the strongly empirical method of Aristotle. Instead of explicitly postulating unobservable entities to explain

the observed world, his basic procedure was to analyse observable things immediately into their parts and principles and then to reconstruct the world rationally from the discovered constituents (see Vol. I, pp. 67–68). This method involved no long chains of deduction such as are found in Euclid, but kept its conclusions as close as possible to things as they were observed.

The history of Greek thought about scientific method can be dramatised by seeing it as an attempt by the mathematicians to impose a clearly postulational scheme, which provoked the resistance of those, especially in medicine, with greater experience of the enigmas of matter. The drama can be followed within the Hippocratic medical writings themselves and continued among the physicists and physiologists of Alexandria. It gave rise at one extreme to an excessive dogmatism about the possibility of discovering causes, and at the other to the sceptical views of the Sophists and the Empirical school of medicine. It continued in the Middle Ages, with the added complication that the translations available did not always allow the actual views of classical writers to be clearly appreciated or respected. Grosseteste notoriously interpreted Aristotle in a Platonic sense and introduced into his logic postulational examples taken from Euclid.

Among ancient Greek writers known in the early 13th century, only Aristotle and certain medical writers, especially Galen, had seriously discussed the inductive and experimental side of science; Aristotle himself was, of course, a doctor. Certain of Aristotle's followers in the Lyceum and in Alexandria, in particular Theophrastus and Strato, had had a very clear understanding of some of the general principles of the experimental method, and experiment seems to have been practised fairly generally by members of the medical school at Alexandria. But the writings of these authors were almost unknown in the Middle Ages. Even in their own time their methods did not have the transforming effect on Greek science which the methods begun in the Middle Ages were to have in the modern world.

Among the Arabs, experiments had been carried out by

a number of scientific writers: for example by Alkindi and Alhazen, al-Shirazi and al-Farisi in optics, and by Rhazes, Avicenna and others in chemistry; and certain Arab medical men, especially Ali ibn Ridwan and Avicenna, had made contributions to the theory of induction. But for one reason or another Arabic science failed to become thoroughly experimental in outlook, though it was certainly the example of Arab work that stimulated some of the experiments made by Christian writers, for instance Roger Bacon and Theodoric of Freiberg and possibly Petrus Peregrinus, discussed on earlier pages.

Before the Greek conception of science had been fully recovered, some Western scholars in the 12th century had shown both that they were aware of the need for proofs in mathematics, even though they could not give them, and that they held at least in principle that nature must be investigated by observation. The saying, *nihil est in intellectu quod non prius fuerit in sensu*, became a commonplace, and such a natural philosopher as Adelard of Bath described simple experiments and may actually have carried out some of them. At the same time scholars gave an increasing value to the practical applications of science and to the accuracy and manual skill found in the practical arts (see Vol. I, p. 175 *et seq.*). By the 13th century the knowledge of the Greek conceptions of theoretical explanation and mathematical proof, gained from the translations of classical and Arabic works, had put philosophers in a position to convert the naïve theoretical empiricism of their predecessors into a conception of science that was both experimental and demonstrative. Characteristically, in receiving ancient and Arabic science into the Western world, they made an attempt not only to master its technical content but also to understand and to prescribe its methods, and so found themselves embarking on a new scientific enterprise of their own.

It must not be supposed that this philosophical conception of experimental science, developed largely in commentaries on Aristotle's *Posterior Analytics* and the problems found in it, was accompanied by a single-minded reliance on the experimental method such as is found in

the 17th century. Medieval science remained in general within the framework of Aristotle's theory of nature, and deductions from that theory were by no means always rejected even when contradicted by the results of the new mathematical, logical, and experimental procedures. Even in the midst of otherwise excellent work, medieval scientists sometimes showed a strange indifference to precise measurements, and could be guilty of misstatements of fact, often based on purely imaginary experiments copied from earlier writers, which the simplest observation would have corrected. Nor must it be supposed that when the new experimental and mathematical methods were applied to scientific problems, this was always the *result* of the theoretical discussions of method. In fact the examples of scientific investigations undertaken in application of a conscious conception of method were often of little scientific interest, whereas some of the most interesting scientific treatises, especially those written in the 13th century, for example those of Jordanus on statics, of Gerard of Brussels on kinematics, of Petrus Peregrinus on magnetism, contain little or no discussion of problems of method. This does not mean that their authors were necessarily uninfluenced by discussions of method; certainly the work of Gerard of Brussels illustrates the influence, not of the ideas of the philosophers, but of the model of Archimedes, the greatest of the Greek mathematical physicists, the role of whose writings in the development of scientific thought in the Middle Ages is still under historical investigation.[1] In the 14th century the influence of philosophical discussions of method on the inquiry into problems is both evident and important. But the examples given do show that in the Middle Ages, as in other periods, discussions of method and actual scientific investigations belonged to two separate streams, even though their waters were so often and so profoundly mingled, as certainly they were in the entire period covered by the pages that follow.

Among the first to understand and use the new theory of experimental science was Robert Grosseteste, who was the

real founder of the tradition of scientific thought in medieval Oxford and, in some ways, of the modern English intellectual tradition. Grosseteste united in his own work the experimental and the rational traditions of the 12th century and he set forth a systematic theory of experimental science. He seems to have studied medicine as well as mathematics and philosophy, so he was well equipped. He based his theory of science in the first place on Aristotle's distinction between knowledge of a fact (*demonstratio quia*) and knowledge of the reason for the fact (*demonstratio propter quid*). His theory had three essentially different aspects which, in fact, characterise all the discussions of methodology down to the 17th century and indeed down to the present day: the inductive, the experimental, and the mathematical.

The problem of induction, Grosseteste held, was to discover the cause from knowledge of the effect. Knowledge of particular physical facts, he said, following Aristotle, was had through the senses, and what the senses perceived were composite objects. Induction involved the breaking up of these objects into the principles or elements that produced them or caused their behaviour, and he conceived of induction as an upward process of abstraction going from what Aristotle had said was 'more knowable to us,' that is, the composite objects perceived through the senses, to abstract principles prior in the order of nature but at first less knowable to us. We must proceed inductively from effects to causes before we can proceed deductively from cause to effect. What had to be done in trying to explain a particular set of observed facts was, therefore, to arrive at a statement or definition of the principle or 'substantial form' that caused them. As Grosseteste wrote in his commentary on Aristotle's *Physics:* 'Since we search for knowledge and understanding by means of principles, in order that we may know and understand natural things we must first determine the principles pertaining to all things. Now the natural way for us to reach knowledge of principles is to start from universal applications and go to these principles, to start from wholes corresponding to these very principles . . . Then as, speaking generally, the procedure

for acquiring knowledge is to go from universal compound wholes to more determined species, so, from complete wholes which we know confusedly . . . we can go back to those very parts by means of which it is possible to define the whole and, from this definition, to reach a determinate knowledge of the whole. . . . Every agent has that which is to be produced, in some way already described and formed within it, and so the "nature" as an agent has the natural things which are to be produced in some way described and formed within itself. This description and form (*descriptio et formatio*), existing in the nature itself of the things to be produced, before they are produced, is therefore called knowledge of this nature.'[2]

All discussions of scientific method must presuppose a philosophy of nature, a conception of the kinds of causes and principles the method will discover. In spite of the Platonic influence shown in the fundamental significance he gave to mathematics in the study of physics, the framework of Grosseteste's philosophy of nature was essentially Aristotelian. He saw the definition of the principles explaining a phenomenon, in effect a definition of the conditions necessary and sufficient to produce it, entirely within the categories of the four Aristotelian causes. As he wrote in *De Natura Causarum* (published by L. Baur in his edition of Grosseteste's philosophical works in *Beiträge zur Geschichte der Philosophie des Mittelalters*, Münster, 1912, vol. 9, p. 121):

> Thus we have four genera of causes and from these, when they exist, there must be a caused thing in its complete being. For a caused thing cannot follow upon the being of any other cause except those four, and that alone is a cause from whose being something else follows. Therefore there is no other cause beyond these, and so there is in these genera a number of causes that is sufficient.

To arrive at such a definition Grosseteste described, first, a dual process which he called 'resolution and composition.'

These names came from the Greek geometers and from Galen and other later classical writers, and were of course simply the Latin translations of the Greek words for 'analysis and synthesis.'[3] The central principle of his method Grosseteste derived, in fact, from Aristotle, but he developed it more fully than Aristotle had done. The method followed a definite order. By the first procedure, resolution, he showed how to sort out and classify, by likeness and difference, the component principles or elements constituting a phenomenon. This gave him what he called the nominal definition. He began by collecting instances of the phenomenon under examination and noting the attributes all had in common, till he arrived at the 'common formula' which stated the empirical connection observed, a causal connection being suspected when attributes were found frequently associated together. Then, by the opposite process of composition, by rearranging the propositions so that the more particular were seen to follow deductively from the more general, he showed that the relation of general to particular was one of cause and effect. That is, he arranged the propositions in causal order. He illustrated his method by showing how to arrive at the common principle causing animals to have horns which, he said in his commentary on the *Posterior Analytics*, book 3, chapter 4, 'is due to the lack of teeth in the upper mandible in those animals to which nature does not give other means of preservation in place of horns,' as she does to the deer with its

rapid flight and to the camel with its large body. In horned animals the earthy matter that would have gone to form the upper teeth went instead to form the horns. He added: 'Not having teeth in both jaws is also the cause of having several stomachs,' a correlation which he traced to the poor mastication of food by animals with only one row of teeth.

Besides this orderly process by which the causal principle was reached by resolution and composition, Grosseteste also envisaged the possibility, as Aristotle had, of a theory or principle explaining repeatedly observed facts being reached by a sudden leap of intuition or scientific imagination. In either case, the further problem then presented itself, namely, how to distinguish between false and true theories. This introduced the use of specially arranged experiments or, where it was not possible to interfere with natural conditions, for example in the study of comets or heavenly bodies, the making of observations that would give the answer to specific questions.

Grosseteste held that it was never possible in natural science to arrive at a complete definition or an absolutely certain knowledge of the cause or form from which effects followed, as it was, for example, with the abstract subjects of geometry like triangles. A triangle could be completely defined by certain of its attributes, for instance by defining it as a figure bounded by three straight lines; from this definition all its other properties could be analytically deduced, so that cause and effect were reciprocal. This was not possible with material subjects because the same effect might follow from more than one cause and it was never possible to know all the possibilities. 'Can the cause be reached from knowledge of the effect in the same way as the effect can be shown to follow from its cause?' he wrote in book 2, chapter 5 of his commentary on the *Posterior Analytics*. 'Can one effect have many causes? For, if one determinate cause cannot be reached from the effect, since there is no effect that has not some cause, it follows that an effect, just as it has one cause, so it may have another, and so there may be several causes of it.' Grosseteste's point seems to be that there may be an ostensible plurality of causes, which our available methods and knowledge

may not enable us to reduce to one actual cause in which the effect is univocally prefigured. In natural science, as he wrote in book 1, chapter 11, owing to the remoteness of causes from immediate observation and to the mutability of natural things, there is thus 'minor certitudo.' Natural science offered its explanations 'probably rather than scientifically . . . Only in mathematics is there science and demonstration in the strictest sense.' It was precisely because they were in the nature of things hidden from our direct inspection that a scientific method was necessary to bring these causes 'more knowable in nature but not to us' as certainly as possible to light. By making deductions from the various theories advanced and by eliminating theories whose consequences were contradicted by experience, it was possible, Grosseteste held, to approach closer to a true knowledge of the causal principles or forms really responsible for events in the world of our observation.

As he said in his commentary on the *Posterior Analytics*, book 1, chapter 14:

> This therefore is the way by which the abstracted universal is reached from singulars through the help of the senses . . . For when the senses several times observe two singular occurrences, of which one is the cause of the other or is related to it in some other way, and they do not see the connection between them, as, for example, when someone frequently notices that the eating of scammony happens to be accompanied by the discharge of red bile, and does not see that it is the scammony that attracts and withdraws the red bile; then, from the constant observation of these two observable things, he begins to form a third unobservable thing, namely, that scammony is the *cause* that withdraws red bile. And from this perception repeated again and again and stored in the memory, and from the sensory knowledge from which the perception is built up, the functioning of the reasoning begins. The functioning reason therefore begins to wonder and to consider whether things really are as the sensible recollection says, and these two

lead the reason to the experiment, namely, that he should administer scammony after all other causes purging red bile have been isolated and excluded. When he has administered scammony many times with the sure exclusion of all other things that withdraw red bile, then there is formed in the reason this universal, namely, that all scammony of its nature withdraws red bile, and this is the way in which it comes from sensation to a universal experimental principle.

His method of elimination or falsification Grosseteste based on two assumptions about the nature of reality. The first was the principle of the uniformity of nature, meaning that forms are always uniform in the effects they produce. 'Things of the same nature are productive of the same operations according to their nature,' he said in his tract *De Generatione Stellarum* (published by Baur in his edition of Grosseteste's philosophical works). Aristotle had stated the same principle. Grosseteste's second assumption was the principle of economy, which he generalised from various statements of Aristotle. This principle Grosseteste used both as describing an objective characteristic of nature and as a pragmatic principle. 'Nature operates in the shortest way possible,' he said in his *De Lineis, Angulis et Figuris,* and he used this as an argument to support the law of reflection of light and his own 'law' of refraction. He said also, in his commentary on the *Posterior Analytics,* book 1, chapter 17:

> that demonstration is better, other circumstances being equal, which necessitates the answering of a smaller number of questions for a perfect demonstration, or requires a smaller number of suppositions and premises from which the demonstration proceeds . . . because it gives us knowledge more quickly.

In the same chapter and elsewhere Grosseteste spoke explicitly of applying the method of *reductio ad absurdum* to the investigation of nature. His method of falsification is an application of this method in an empirical situation. He used it explicitly in several of his scientific opuscula

where it was appropriate, for instance in his studies on the nature of the stars, on comets, on the sphere, on heat, and on the rainbow. A good example is in the tract *De Cometis*, in which he considered in turn four different theories put forward by earlier writers to account for the appearance of comets. The first was that put forward by observers who thought that comets were produced by the reflection of the sun's rays falling on a heavenly body. This hypothesis was falsified, he said, by two considerations: first, in terms of another physical theory, because the reflected rays would not be visible unless they were associated with a transparent medium of a terrestrial and not a celestial nature; and secondly, because it was observed that

> the tail of the comet is not always extended in the op-
> posite direction to the sun, whereas all reflected rays
> would go in the opposite direction to the incident rays
> at equal angles.[4]

He considered the other hypotheses in the same way in terms of 'reason and experience,' rejecting those contrary either to what he regarded as an established theory verified by experience or to the facts of experience (*ista opinio falsificatur*, as he said), till he came to his final definition which he held survived these tests, that 'a comet is sublimated fire assimilated to the nature of one of the seven planets.' This theory he then used to explain various further phenomena, including the astrological influence of comets.

Of even greater interest is the method Grosseteste used in his attempt to explain the shape of the rainbow (see Vol. I, p. 103), when he seized upon simpler phenomena which could be investigated experimentally, the reflection and refraction of light, and tried to deduce the appearance of the rainbow from the results of a study of these.

In fact the tails of comets are repelled by the sun, though the angles would differ from those made by reflected light. Good examples of the same kind of empirical analysis are Aristotle's discussions of comets in the *Meteorology* (book 1, chapter 6) and his refutation of pangenesis in *De Generatione Animalium* (book 1, chapters 17, 18).

Grosseteste's own work on the rainbow is somewhat elementary, but the experimental investigation of the subject which Theodoric of Freiberg undertook is truly remarkable both for its precision and for the conscious understanding he shows of the possibilities of the experimental method (see Vol. I, p. 110 *et seq.*). The same characteristics are to be found in the work of other experimental scientists who came after Grosseteste, for instance in that of Albertus Magnus, Roger Bacon, Petrus Peregrinus, Witelo and Themon Judæi, even though nearly all these writers could also be guilty of elementary mistakes. The influence of Grosseteste is especially noticeable on those who studied the rainbow. For example the initial inquiries of Roger Bacon and Witelo were aimed at discovering the conditions necessary and sufficient to produce this phenomenon. The 'resolutive' part of their investigations gave them a partial answer by defining the species to which the rainbow belonged and by distinguishing it from the species to which it did not belong. It belonged to a species of spectral colours produced by the differential refraction of sunlight passing through drops of water; as Bacon pointed out, this was different, for example, from the species including the colours seen in iridescent feathers. Moreover a further defining attribute of the rainbow was that it was produced by a large number of discontinuous drops. 'For,' as Themon wrote in his *Quæstiones super Quatuor Libros Meteorum*, book 3, question 14, 'where such drops are absent there no rainbow or part of it appears, although all the other requisite conditions are sufficient.' This, he said, could be tested by experiments with rainbows in artificial sprays. Roger Bacon had made such experiments. Postulating the requisite conditions—the sun at a definite position in relation to raindrops and to the observer—a rainbow would result.

Having defined these conditions, the purpose of the next stage of the inquiry was to discover how they would in fact produce a rainbow; that is, to construct a theory incorporating them in such a manner that a statement describing the phenomena could be deduced from it. The two essential problems were to explain, first, how the col-

ours were formed by the raindrops, and secondly, how they were sent back to the observer in the shape and order seen. Particularly significant features of the whole inquiry were the use of model raindrops in the shape of spherical flasks of water, and the procedures of verification and falsification to which each theory was submitted, especially by the authors of rival theories. For example, the discovery of the differential refraction of the colours having pointed the way to the solution of the first problem, Witelo tried to solve the second by supposing that the sunlight was refracted right through one raindrop and the resulting colours then reflected back to the observer from the convex external surfaces of other drops behind. Theodoric of Freiberg showed that this theory would not give the observed effects, but that these would follow from the theory he based on his own discovery of the internal reflection of light within each drop. Thus by theory and experiment he solved the problem he set himself. For, as he wrote in the preface to *De Iride*, 'it is the function of optics to determine what the rainbow is, because, in doing so, it shows the reason for it, in so far as there is added to the description of the rainbow the manner in which this sort of concentration may be produced in the light going from any luminous heavenly body to a determined place in a cloud, and then by particular refractions and reflections of rays is directed from that determined place to the eye.'

Different from, though in many cases (as indeed in the case of Galileo himself) scarcely to be separated from, the experimental method and the making of special observations to verify or falsify theories, was the use of mathematics in natural science. Grosseteste himself, because of his 'cosmology of light' (see Vol. I, pp. 74, 99 *et seq.*), said in his little work, *De Natura Locorum*, that from the 'rules and principles and fundamentals . . . given by the power of geometry, the careful observer of natural things can give the cause of all natural effects.' And elaborating this idea in his *De Lineis* he said:

The usefulness of considering lines, angles and figures is the greatest because it is impossible to understand
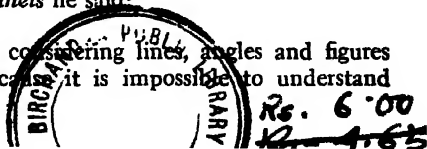
natural philosophy without these . . . For all causes of natural effects have to be expressed by means of lines, angles and figures, for otherwise it would be impossible to have knowledge of the reason for those effects.

Grosseteste, in fact, regarded the physical sciences as being subordinate to the mathematical sciences in the sense that mathematics could provide the reason for observed physical facts, though, at the same time, he maintained the Aristotelian distinction between the mathematical and the physical propositions in a given theory, and asserted the necessity of both for a complete explanation. Essentially the same attitude was taken by many leading scientists throughout the Middle Ages, and, indeed, in a different form by most writers in the 17th century. Mathematics could describe what happened, could correlate the concomitant variations in the observed events, but it could not say anything about the efficient and other causes *producing* the movement because it was, in fact, explicitly an abstraction from such causes (see Vol. I, p. 74). This is the attitude seen in both optics and astronomy in the 13th century (see Vol. I, pp. 100, 78 *et seq.*).

As time went on, the retention of causal, 'physical' explanations, which usually meant explanations taken from Aristotle's qualitative physics, became more and more of an embarrassment. The great advantage of mathematical theories was just that they could be used to correlate concomitant variations in a series of observations made with measuring instruments so that the truth or falsehood of these theories, and the precise occasions where they failed, could easily be determined experimentally. It was just this consideration which brought about the triumph of Ptolemaic over the Aristotelian astronomy at the end of the 13th century (see Vol. I, p. 89). In contrast with this clearly understood role of mathematics in a scientific investigation, it was difficult to see what to do with a theory of 'physical' causes, however necessary they might seem to be theoretically for a complete explanation of the observed occurrences. Moreover, many aspects of Aristotle's physical philosophy were a positive hindrance to the use of

mathematics. From the beginning of the 14th century attempts were made to circumvent these difficulties by devising new systems of physics, partly under the influence of a revived Neoplatonism and partly under the influence of the 'nominalism' revived by William of Ockham.

Improvements were made in the theory of induction by several writers after Grosseteste and the enormous and sustained interest taken in this purely theoretical and logical question is a good indication of the intellectual climate in which natural science was conducted before the middle of the 17th century. Perhaps it does something to explain why the brilliant beginnings of experimental science seen in the 13th and early 14th centuries did not at once go on to bring about what, in fact, only happened in the 17th century. For some four centuries from the beginning of the 13th century, the question guiding scientific inquiry was to discover the real, the enduring, the intelligible behind the changing world of sensible experience, whether this reality was something qualitative, as it was conceived of at the beginning of that period, or something mathematical, as Galileo and Kepler were to conceive of it at the end. Some aspects of this reality might be revealed by physics or natural science, others by mathematics, others again by metaphysics; yet though these different aspects were all aspects of a single reality, they could not all be investigated in the same way or known with the same certainty. For this reason it was essential to be clear as to the methods of investigation and explanation legitimate in each case, and what each could reveal of the underlying reality. In most scientific writings down to the time of Galileo a discussion of methodology is carried on *pari passu* with the account of a concrete investigation, and this was a necessary part of the endeavour of which modern science is the result. But from the beginning of the 14th century to the beginning of the 16th there was a tendency for the best minds to become increasingly interested in problems of pure logic divorced from experimental practice, just as in another field they became more interested in making purely theoretical, though also necessary, criticisms of

Aristotle's physics without bothering to make observations (see below, p. 35 *et seq.*).

Perhaps the first writer after Grosseteste seriously to discuss the problem of induction was Albertus Magnus. He had a good grasp of the general principles as they were then understood, but of greater interest is the work done by Roger Bacon. In chapter 2 of the sixth part of his *Opus Majus*, 'On Experimental Science,' Bacon said:

> This experimental science has three great prerogatives with respect to the other sciences. The first is that it investigates by experiment the noble conclusions of all the sciences. For the other sciences know how to discover their principles by experiments, but their conclusions are reached by arguments based on the discovered principles. But if they must have particular and complete experience of their conclusions, then it is necessary that they have it by the aid of this noble science. It is true, indeed, that mathematics has universal experiences concerning its conclusions in figuring and numbering, which are applied likewise to all sciences and to this experimental science, because no science can be known without mathematics. But if we turn our attention to the experiences which are particular and complete and certified wholly in their own discipline, it is necessary to go by way of the considerations of this science which is called experimental.

The first prerogative of Roger Bacon's experimental science was thus to confirm the conclusions of mathematical reasoning; the second was to add to deductive science knowledge at which it could not itself arrive, as, for instance, in alchemy; and the third was to discover departments of knowledge still unborn. His experimental science was, he admitted, as much a separate applied science, in which results of the natural and speculative sciences were put to the test of practical utility, as an inductive method. His attempt to discover the cause of the rainbow (see Vol. I, pp. 105–6), with which he illustrated the first prerogative of experimental science, shows that he had grasped

the essential principles of induction by which the investigator passed from observed effects to the discovery of the cause and isolated the true cause by eliminating theories contradicted by facts.

With Roger Bacon the programme for mathematicising physics and a shift in the object of scientific inquiry from the Aristotelian 'nature' or 'form,' to laws of nature in a recognisably modern sense, becomes explicit (cf. below, p. 85 *et seq.*). Echoing Grosseteste, he wrote for example in the *Opus Majus*, part 4, distinction 4, chapter 8: 'In the things of this world, as regards their efficient and generating causes, nothing can be known without the power of geometry.' The language he used in discussing the 'multiplication of species' seems to relate this general programme unequivocally to the inquiry for predictive laws. In *Un fragment inédit de l'Opus Tertium* edited by Duhem (p. 90), he wrote: 'That the laws (*leges*) of reflection and refraction are common to all natural actions I have shown in the treatise on geometry.' He claimed to have demonstrated the formation of the image in the eye 'by the law of refraction,' remarking that the 'species of the thing seen' must so propagate itself in the eye 'that it does not transgress the laws which nature keeps in the bodies of the world.' Normally the 'species' of light were propagated in straight lines, but in the twisting nerves 'the power of the soul makes the species relinquish the common laws of nature (*leges communes naturæ*) and behave in a way that suits its operations.' (Ibid., p. 78.)

For some three hundred years from the middle of the 13th century a most interesting series of discussions of induction was made by members of the various medical schools, and in this the tendency towards pure logic becomes very marked. Galen himself had recognised the need for some method of discovering the causes which explained the observed effects, when he drew a distinction between the 'method of experience' and the 'rational method.' He referred to effects or symptoms as 'signs,' and he said that the 'method of experience' was to go inductively from these signs to the causes which produced them, and that this method necessarily preceded the 'rational method' which

demonstrated syllogistically[5] from causes to effects. Galen's ideas had been developed by Avicenna in his *Canon of Medicine* and this work contained an interesting discussion of the conditions which must be observed in inducing the properties of medicines from their effects. The subject was taken up in the 13th century by the Portuguese doctor Petrus Hispanus, who died in 1277 as Pope John XXI, in his *Commentaries on Isaac*, a work on diets and medicines. First, he said, the medicine administered should be free from all foreign substances. Secondly, the patient taking it should have the disease for which it was especially intended. Thirdly, it should be given alone without admixture of other medicine. Fourthly, it should be of the opposite degree to the disease.[6] Fifthly, the test should be made not once only but many times. Sixthly, the experiments should be with the proper body, on the body of a man and not of an ass. On the fifth of these conditions a contemporary, John of St. Amand, repeated the warning that a medicine which had had a heating effect on five men would not necessarily always have a heating effect, for the men in question might all have been of a cold and temperate constitution, whereas a man of hot nature would not find the medicine heating.

From the beginning of the 14th century the subject of induction was taken up in the medical school of Padua where, owing to the influence of the Averroïsts who had come to dominate the university, the philosophical climate was thoroughly Aristotelian. From the time of Pietro

---

[5] The syllogism is a form of reasoning in which, from two given propositions, the premisses, with a common or middle term, is deduced a third proposition, the conclusion, in which the non-common terms are united. For example, from the major premiss, 'whatever has an opaque body interposed between it and its source of light loses its light,' and the minor premiss, 'the moon has an opaque body interposed between it and its source of light,' the conclusion follows, 'therefore the moon loses its light,' that is, suffers an eclipse. In this way an eclipse of the moon is explained as an instance of a more general principle.

[6] I.e., if the disease causes an excess of one quality such as heat the medicine should cause a decrease in that quality, that is, have a cooling effect (cf. Vol. I, p. 163 *et seq.*).

d'Abano in his famous *Conciliator* in 1310, down to Zaba-
rella in the early 16th century, these medical logicians de-
veloped the methods of 'resolution and composition' into
a theory of experimental science very different from the
method simply of observing ordinary, everyday occurrences
with which Aristotle and some of the earlier scholastics
had been content to verify their scientific theories. Starting
from observations, the complex fact was 'resolved' into its
component parts:

> the fever into its causes, since any fever comes either
> from the heating of the humour or of the spirits or of
> the members; and again the heating of the humour is
> either of the blood or of the phlegm, etc.: until you
> arrive at the specific and distinct cause and knowledge of
> that fever,

as Jacopo da Forli (d. 1413) said in his commentary *Super
Tegni Galeni,* comm. text 1. A hypothesis was then ex-
cogitated from which the observations could again be de-
duced, and these deduced consequences suggested an ex-
periment by which the hypothesis could be verified. This
method was followed by doctors of the period in the au-
topsies performed to discover the origin of a disease or the
causes of death, and in the clinical study of medical and
surgical cases recorded in their *consilia*. It has been shown
that Galileo himself derived much of the logical structure
of his science from his Paduan predecessors, whose techni-
cal terms he used (see below, p. 136 *et seq.*), though he
did not go so far as to accept the conclusion of a late mem-
ber of this school, Agostino Nifo, who said (1506), that
since the hypotheses of natural science rested simply on the
facts they served to explain, therefore all natural science
was merely conjectural and hypothetical. The double pro-
cedure of resolution and composition was given in Padua
the Averroïst name *regressus*. Discussing this 'regress,' be-
ginning with the inquiry for the cause of an observed effect,
Nifo wrote in his *Expositio super Octo Aristotelis Libros
de Physico,* published at Venice in 1552, book 1, com-
mentary 4:

When I more diligently consider the words of Aristotle, and the commentaries of Alexander and Themistius, of Philoponus and Simplicius, it seems to me that in the regress made in demonstrations in natural science the first process, by which the discovery of the cause is put into syllogistic form, is a mere hypothetical (*coniecturalis*) syllogism. . . . But the second process, by which is syllogised the reason why the effect is so through the discovered cause, is demonstration *propter quid*–not that it makes us know *simpliciter*, but conditionally (*ex conditione*), provided that that really is the cause, or provided that the propositions are true that represent it to be the cause, and that nothing else can be the cause. . . . Alexander . . . asserts that the discovery of the circles of epicycles and eccentrics from the appearances which we see is conjectural . . . The opposite process he says to be a demonstration, not because it makes us know *simpliciter*, but conditionally, provided that those really are the cause and that nothing else can be the cause: for if those exist, then so do the appearances, but whether anything else can be the cause is not known to us *simpliciter*. . . . But you object that in that case the science of nature is not a science at all. To that it can be replied that the science of nature is not a science *simpliciter*, like mathematics. Yet it is a science *propter quid*, because the discovered cause, gained through a conjectural syllogism, is the reason why the effect is so . . . That something is a cause can never be so certain as that an effect exists (*quia est*); for the existence of an effect is known to the senses. That it is the cause remains conjectural . . .

The whole of the pre-Galilean tradition of scientific method at Padua was finally summed up by Jacopo Zabarella (1533–89) in a series of treatises on the subject. Sharing the conception that had been developing since the 13th century that natural scientific explanations were hypothetical, he wrote in chapter 2 of *De Regressu*: 'demonstrations are made by us and for us ourselves, not for nature.' And he went on in chapter 5:

There are, I judge, two things that help us to know the cause distinctly. One is the knowledge *that* it is, which prepares us to discover *what* it is. For when we form some hypothesis about the matter we are able to search out and discover something else in it; where we form no hypothesis at all, we shall never discover anything . . . Hence when we find that cause to be suggested, we are in a position to seek out and discover what it is. The other help, without which this first would not suffice, is the comparison of the cause discovered with the effect through which it was discovered, not indeed with the full knowledge that this is the cause and that the effect, but just comparing this thing with that. Thus it comes about that we are led gradually to the knowledge of the conditions of that thing; and when one of the conditions has been discovered we are helped to the discovery of another, until we finally know this to be the cause of that effect. . . . The regress thus consists necessarily of three parts. The first in a 'demonstration that,' by which we are led from a confused knowledge of the effect to a confused knowledge of the cause. The second is this 'mental consideration' by which, from a confused knowledge of the cause, we acquire a distinct knowledge of it. The third is demonstration in the strictest sense, by which we are at length led from the cause distinctly known to the distinct knowledge of the effect. . . . From what we have said it can be clear that it is impossible to know fully that this is the cause of this effect, unless we know the nature and conditions of this cause, by which it is capable of producing such an effect.

Of great importance for the whole of natural science were the discussions of induction made by two Franciscan friars of Oxford living at the end of the 13th and the beginning of the 14th centuries. With them, and particularly with the second of them, began the most radical attack on Aristotle's system from a theoretical point of view. Both were preoccupied with the natural grounds of certainty in knowledge and the first, John Duns Scotus (*c.* 1266–1308), may be considered as summing up the tradition of Oxford

thought on 'theory of science' which began with Grosse-teste, before that tradition was projected violently in new directions by his successor, William of Ockham (c. 1284–1349). Each of them set out his essential point of view early in life in a theological work, their commentaries on the *Sentences* of Peter Lombard.

The principal contribution made by Scotus to the problem of induction was the very clear distinction he drew between causal laws and empirical generalisations. Scotus said that the certainty of the causal laws discovered in investigating the physical world was guaranteed by the principle of the uniformity of nature, which he regarded as a self-evident assumption of inductive science. Even though it was possible to have experience of only a sample of the correlated events under investigation, the certainty of the causal connection underlying the observed correlation was known to the investigator, he said (in his *Oxford Commentary*, book 1, distinction 3, question 4, article 2), 'by the following proposition reposing in the soul: *Whatever occurs as in a great many cases from some cause which is not free* [*i.e.*, not free-will] *is the natural effect of that cause.*' The most satisfactory scientific knowledge was that in which the cause was known, as, for instance, in the case of an eclipse of the moon deducible from the proposition: 'an opaque object interposed between a luminous object and an illuminated object impedes the transmission of light to such an illuminated object.' Even when the cause was not known and 'one must stop at some truth which holds as in many cases, of which the extreme terms [of the proposition] are frequently experienced united, as, for example, that a herb of such and such a species is hot'—even, that is, when it was impossible to get beyond an empirical generalisation—the certainty that there was a causal connection was guaranteed by the uniformity of nature.

William of Ockham, on the other hand, was sceptical about the possibility of ever knowing particular causal connections or ever being able to define particular substances, though he did not deny the existence of causes or of substance as the identity persisting through change. He believed, in fact, that empirically established connections

had a universal validity by reason of the uniformity of na-
ture, which he held, like Scotus, to be a self-evident as-
sumption of inductive science. His importance in the
history of science comes partly from some improvements
he introduced into the theory of induction, but much more
from the attack he made on contemporary physics and
metaphysics as a result of the methodological principles
which he adopted.

His treatment of induction Ockham based on two prin-
ciples. First, he held that the only certain knowledge about
the world of experience was what he called 'intuitive
knowledge' gained by the perception of individual things
through the senses. Thus, as he said in the *Summa Totius
Logicæ*, part 3, part 2, chapter 10, 'when some sensible
thing has been apprehended by the senses . . . the intellect
also can apprehend it,' and only propositions about individ-
ual things so apprehended were included in what he called
'real science.' All the rest, all the theories constructed to
explain the observed facts, comprised 'rational science,' in
which names stood merely for concepts and not for any-
thing real.

Ockham's second principle was that of economy, the so-
called 'Ockham's razor.' This had already been stated by
Grosseteste, and Duns Scotus and some other Oxford Fran-
ciscans had said that it was 'futile to work with more enti-
ties when it was possible to work with fewer.' Ockham
expressed this principle in various ways throughout his
works, a common form being one that was used in his
Quodlibeta Septem, quodlibet 5, question 5: 'A plurality
must not be asserted without necessity.' The well-known
phrase *Entia non sunt multiplicanda præter necessitatem*
was introduced only in the 17th century by a certain John
Ponce of Cork, who was a follower of Duns Scotus.

The improvements Ockham made in the logic of induc-
tion were based principally on his recognition of the fact
that 'the same species of effect can exist through many
different causes,' as he said in the same chapter of the
Summa Totius Logicæ as quoted from above. To estab-
lish causal connections in particular cases he formulated
rules, as in the following passage from his *Super Libros*

Quatuor Sententiarum, book 1, distinction 45, question 1, D:

> Although I do not intend to say universally what an immediate cause is, nevertheless I say that this is sufficient for something being an immediate cause, namely that when it is present the effect follows, and when it is not present, all the other conditions and dispositions being the same, the effect does not follow. Whence everything that has such a relation to something else is an immediate cause of it, although perhaps not *vice versa*. That this is sufficient for anything being an immediate cause for anything else is clear, because if not there is no other way of knowing that something is an immediate cause of something else. . . . It follows that if, when either the universal or the particular cause is removed, the effect does not occur, then neither of them is the total cause but rather each a partial cause, because neither of those things from which by itself alone the effect cannot be produced is the efficient cause. and consequently neither is the total cause. It follows also that every cause properly so called is an immediate cause, because a so-called cause that can be absent or present without having any influence on the effect. and which when present in other circumstances does not produce the effect, cannot be considered a cause; but this is how it is with every other cause except the immediate cause, as is clear inductively.

This amounts to something like J. S. Mill's Method of Agreement and Difference. Since the same effect might have different causes, it was necessary to eliminate rival hypotheses. 'So,' said Ockham in the same work, prologue, question 2, G:

> let this be posited as a first principle all herbs of such and such species cure a fevered person. This cannot be demonstrated by syllogism from a better-known proposition, but it is known by intuitive knowledge and perhaps of many instances. For since he observed that after eating such herbs the fevered person was cured and he re-

moved all other causes of his recovery, he knew evidently that this herb was the cause of recovery, and then he has experimental knowledge of a particular connection.

Ockham denied that it could be proved either from first principles or from experience that any given effect had a final cause. 'The special characteristic of a final cause,' he said in his *Quodlibeta Septem*, quodlibet 4, question 1, 'is that it is able to cause when it does not exist'; 'from which it follows that this movement towards an end is not real but metaphorical,' he concluded in his *Super Quatuor Libros Sententiarum*, book 2, question 3, G. This phrase was in fact a commonplace and was used for example by Albertus Magnus and Roger Bacon. But for Ockham only immediate or proximate causes were real, and the 'total cause' of an event was the aggregate of all the antecedents which sufficed to bring about the event.

The effect of Ockham's attack on contemporary physics and metaphysics was to destroy belief in most of the principles on which the 13th-century system of physics was based. In particular, he attacked the Aristotelian categories of 'relation' and 'substance' and the notion of causation. He held that relations, such as that of one thing being above another in space, had no objective reality apart from the individual perceptible things between which the relation was found. Relations, according to him, were simply concepts formed by the mind. This view was incompatible with the Aristotelian idea of the cosmos having an objective principle of order according to which its constituent substances were arranged, and it opened the way for the notion that all motion was relative in an indifferent geometrical space without qualitative differences.

In discussing 'substance,' Ockham said that experience was had only of attributes and that it could not be demonstrated that any given observed attributes were caused by a particular 'substantial form.' He held that the regular sequences of events were simply sequences of fact, and that the primary function of science was to establish these sequences by observation. It was impossible to be certain about any particular causal connections, for experience

gave evident knowledge only of individual objects or events and never of the relation between them as cause and effect. For example, the presence of fire and the sensation of burning were found associated together, but it could not be demonstrated that there was any causal connection between them. It could not be proved that any particular man was a man and not a corpse manipulated by an angel. In the natural course of things a sensation was had only from an existing object, but God could give us a sensation without an object. This attack on causation was to lead Ockham to make revolutionary statements on the subject of motion (see below, pp. 62–66).

An even greater degree of philosophical empiricism, and one not to be attained again until the writings of David Hume in the 18th century, was reached by a French contemporary of Ockham, Nicholas of Autrecourt (d. after 1350). He doubted the possibility of knowing the existence of substance or causal relations at all. As with Ockham, from a limitation of evidential certitude to what was known through 'intuitive experience' and logically necessary implications, he concluded, in a passage published by J. Lappe in *Beiträge zur Geschichte der Philosophie des Mittelalters* (1908, vol. 6, part 2, p. 9*): 'from the fact that one thing is known to exist, it cannot be evidently inferred that another thing exists,' or does not exist; from which it followed that from a knowledge of attributes it was impossible to infer the existence of substances. And, he said in the *Exigit ordo Executionis*, edited by J. R. O'Donnell in *Mediæval Studies* (1939, vol. 1, p. 237):

concerning things known by experience in the manner in which it is said to be known that rhubarb cures cholera or that the magnet attracts iron, we have only a conjecturative habit (*solum habitus conjecturativus*) but not certitude. When it is said that we have certitude concerning such things in virtue of a proposition reposing in the soul that *that which occurs as in many cases from an unfree course is the natural effect of it*, I ask what you call a natural cause, *i.e.*, do you say that that which produced in the past as in many cases and up to

the present will produce in the future if it remains and is applied? Then the minor [premiss] is not known, for allowing that something was produced as in many cases, it is nevertheless not known that it ought to be thus produced in the future.

And so, he said, in a passage published by Hastings Rashdall in the *Proceedings of the Aristotelian Society*, N.S. vol. 7:

Whatever conditions we take which may be the cause of any effect, we do not evidently know that, when those conditions are posited, the effect posited will follow.

The effect on philosophy in general of this search for evident knowledge was to divert interest within the discussions of the schools away from the traditional problems of metaphysics to the world of experience. Ockhamite nominalism or, as it may more properly be called, 'terminism,' went to show that in the natural world all was contingent and therefore that observations were necessary to discover anything about it.

The relation of faith to reason remained a central problem in medieval speculation, and a diversity of attitudes was taken to it by Augustinians, Thomists, Averroïsts and Ockhamites. 'The spirit and the enterprise' of early medieval philosophy was, as R. McKeon put it in his *Selections from Medieval Philosophers* (vol. 2, pp. ix–x), 'of faith engaged in understanding itself.' Between Augustine and Aquinas philosophy had passed from the consideration of truth as a reflection of God to truth in the relation of things to each other and to man, leaving their relation to God for theology. Ockham himself firmly divorced theology from philosophy, the former deriving its knowledge from revelation and the latter from sensory experience, from which alone it took its origin. And whereas the Averroïsts were driven to entertain the possibility of 'double truth' (see Vol. I, p. 64), the Ockhamites, for instance Nicholas of Autrecourt, sought a solution to the problem in their doctrine of 'probabilism.' By this they meant that natural philosophy could offer a probable but not a neces-

sary system of explanations, and that where this probable system contradicted the necessary propositions of revelation, it was wrong. In his own attempt to reach the most probable system of physics Nicholas made a thoroughgoing attack on the Aristotelian system and arrived at the conclusion that the most probable system was one based on atomism. After this time, no further attempts were made to construct systems rationally synthesising the contents of both faith and reason. Instead, there began a period of increasing reliance on the literal word of the Bible instead of the teaching of a divinely instituted Church, a period of speculative mysticism seen in Eckhart (c. 1260–1327) and Henry Suso (c. 1295–1365), and of empiricism and scepticism seen in Nicholas of Cusa (1401–64) and Montaigne (1533–92). Nicholas of Cusa, for example, held that though it was possible to approach closer and closer to truth, it was never possible to grasp it finally, just as it was possible to draw figures approximating more and more closely to a perfect circle, yet no figure we drew would be so perfect that a more perfect circle could not be drawn. Montaigne was even more sceptical. Indeed, since the 14th century the stream of sceptical empiricism has flowed strongly in European philosophy, and it has done its work of directing attention to the conditions of human knowledge which has produced some of the most important clarifications of scientific methodology.

(2)   MATTER AND SPACE IN LATE MEDIEVAL PHYSICS

The most radical attacks made in the 14th century on Aristotle's whole system of physics concerned his doctrines about matter and space, and about motion. Aristotle had denied the possibility of atoms, void, infinity and plural worlds, but when his strict determinism had been condemned by the theologians in 1277 this opened the way to speculation on these subjects. With the assertion of God's omnipotence, philosophers argued that God could create a

body moving in empty space or an infinite universe and proceeded to work out what the consequences would be if He did. This seems a strange way to approach science, but there is no doubt that it was science they were approaching. They discussed the possibility of plural worlds, the two infinities, and centre of gravity; and they discussed also the acceleration of freely falling bodies, the flight of projectiles, and the possibility of the earth's having motion. Not only did the criticisms of Aristotle remove many of the meta-physical and 'physical' restrictions his system had placed on the use of mathematics, but also many of the new concepts reached were either incorporated directly into 17th-century mechanics or were the germs of theories to be expressed in the new language created by mathematical and experimental techniques.

Central to the whole discussion of matter, space and gravitation in the 13th and 14th centuries were the two conceptions of dimensionality coming respectively from the atomists and Plato, and from Aristotle (cf. Vol. I, pp. 30–32, 73–77). In the *Timæus* Plato had put forward a clearly mathematical conception of space, which he conceived as dimensions independent of bodies but in which bodies could exist and could move; space was in fact the receptacle of all things, as real as the eternal ideas and more real than the bodies occupying it. The part of space occupied by the dimensions of a body was the body's 'place'; the part not so occupied was a vacuum. This was essentially the atomist's view.

To this view Aristotle objected in his *Physics* (book 4) that dimensions could not exist apart from bodies with dimensions; he conceived dimensions as quantitative attributes of bodies, and no attribute could exist apart from the substance in which it inhered (cf. Vol. I, pp. 68–69). Moreover, Aristotle maintained that the conception of space held by Plato and the atomists was useless in explaining the actual movements of bodies: for example, why should a given body go up rather than down, or *vice versa?* His own explanation of the different movements actually observed in bodies was in terms of his conception of 'place.' This had two essential characteristics. Primarily it was the

physical environment of the body, the 'innermost bound-ary' of whatever contained the body. Aristotle maintained that the bodies making up the universe were all contiguous with each other, thus composing a *plenum*. The innate preference of a body for a particular physical environment within this *plenum* was the cause of the natural motions all bodies were observed to have (cf. Vol. I, pp. 69–71, 114-15). To this notion of place as a physical ambience moving each body according to its nature by final causality, Aristotle added also a geometrical, spatial characteristic. He held that each place in the universe was itself motion-less; and in his *De Cælo* he gave to each of the places making up the universe as a whole a position in absolute space relative to the centre of the earth fixed at the centre of the universe. This gave him his conception of 'up' and 'down' as absolute directions from the centre to the circum-ference of the outermost sphere.

Aristotle's conceptions of dimensionality and of place are good examples of the empirical concreteness so notice-able in all his thought. Much of the character of 14th-century physics is a result of a renewed application of the more abstract thinking of Plato and the atomists.

The form of atomism found in Plato's *Timæus* and Lu-cretius' *De Rerum Natura* (see below, p. 105), and in the works of several other ancient Greek writers,[7] had been

[7] The development of the atomic theory in the Ancient World after the time of Plato and Aristotle (for development down to Plato see note in Vol. 1, p 28) was largely the work of Epicurus (340–270 B.C.), Strato of Lampsacus (*fl.c.* 288 B.C.), Philo of Byzantium (2nd century B.C.), and Hero of Alexandria (1st century B.C.). The theory of Epicurus was expounded by Lucretius (c. 95–55 B.C.) in his poem, *De Rerum Natura*. Epicurus made two changes in Democritus's theory. He held, first, that the atoms fell perpendicularly in empty space owing to their weight and secondly, that interactions between them which resulted in the formation of bodies took place as a result of 'swerves' which occurred by chance and led to collisions. He assumed a limited number of shapes but an infinite number of atoms of each shape. Different kinds of atoms had different weights, but all fell with the same velocity. Epicurus also stated a principle which had been held by certain previous atomists, namely, that all bodies of any weight whatever would fall in a void with the same velocity. Differences in velocity of given bodies in a given medium, e.g., air, were due to differences in the

developed by some 13th-century philosophers. Grosseteste, for example, had said that the finite space of the world was produced by the infinite 'multiplication' of points of light, and he also regarded heat as due to a scattering of molecular parts consequent on movement. Even Roger Bacon, though he followed Aristotle and tried to show that atom-

---

proportion of resistance to weight. On collision, atoms became interlocked by little branches or antlers; only the atoms of the soul were spherical. To meet Aristotle's objection based on the change of properties in compounds, he assumed that a 'compound body' formed by the association of atoms could acquire new powers not possessed by individual atoms. The infinite number of atoms produced an infinite number of universes in infinite space. It seems that Strato's treatise *On the Void* was the basis of the introduction to Hero's *Pneumatica*. Strato combined atomist with Aristotelian conceptions and took an empirical view of the existence of void, which he used to explain the differences in density between different bodies. In this he was followed by Philo in his *De Ingeniis Spiritualibus* (which was not widely known in the Middle Ages) and by Hero, who denied the existence of a continuous extended vacuum but made use of interstitial vacua between the particles of bodies to explain the compressibility of air, the diffusion of wine into water, and similar phenomena. These writers also carried out experiments to demonstrate the impossibility of an extended void. Aristotle had proved that air had body by showing that a vessel must be emptied of air before it could be filled with water. Philo and Hero both performed the experiment, also described by Simplicius, showing that in a water clock or clepsydra, water could not *leave* a vessel unless there was a means for air to enter it. Philo also described two other experiments proving the same conclusion. He fixed a tube to a globe containing air and dipped the end of the tube under water, and showed that when the globe was heated air was expelled and when it cooled the contracting air drew water up the tube after it. The air and water remained in contact, preventing a vacuum. He also showed that when a candle was burnt in a vessel inverted over water, the water rose as the air was used up. Apart from these and some other Alexandrian writers, such as the doctor Erasistratus and members of the Methodical sect, atomism was not favourably regarded in antiquity. It was opposed by the Stoics, although they believed in the possibility of void within the universe and in an infinite void beyond its boundaries; and it was opposed also by a number of other writers such as Cicero, Seneca, Galen and St. Augustine. But atomism was briefly discussed by Isidore of Seville, Bede, William of Conches and several Arab and Jewish writers such as Rhazes (d. *c.* 924) and Maimonides (1135-1204).

ism led to consequences which contradicted the teachings of mathematics, for instance the incommensurability of the diagonal and side of a square (see Vol. I, p. 28, note), agreed with Grosseteste in regarding heat as a form of violent motion. Towards the end of the 13th century several writers adopted atomist propositions, though these were refuted by Scotus while discussing the question whether angels could move from place to place with continuous movement. Similar propositions were refuted again early in the 14th century by Thomas Bradwardine (c. 1295–1349). The propositions refuted were that continuous matter consisted either of *indivisibilia*, that is, discontinuous atoms separated from each other, or of *minima*, that is, atoms joined to each other continuously, or of an infinite number of actually existing points.

At the turn of the 13th century, a complete form of atomism was put forward by Giles of Rome (1247–1316), who derived the basis of it from Avicebron's theory of matter as extension successively specified by a hierarchy of forms (see Vol. I, p. 73). Giles held that magnitude might be considered in three ways: as a mathematical abstraction, and as realised in an unspecified and in a specified material substance. An abstract cubic foot and a cubic foot of unspecified matter were then potentially divisible to infinity, but in the division of a cubic foot of water a point was reached at which it ceased to be water and became something else. The geometrical arguments against the existence of natural *minima* were therefore irrelevant. Nicholas of Autrecourt was led, by the impossibility of demonstrating that there was in a piece of bread anything beyond its sensible accidents, to abandon altogether the explanation of phenomena in terms of substantial forms and to adopt a completely Epicurean physics. He came to the probable conclusion that a material *continuum* was composed of minimal, infra-sensible indivisible points, and time of discrete instants, and he asserted that all change in natural things was due to local motion, that is, to the aggregation and dispersal of particles. He also believed that light was a movement of particles with a finite velocity. That some of these conclusions were proposed with reference to

a discussion of the theological doctrine of transubstantiation shows how closely all cosmological questions were linked together, and was one reason why he was obliged to retract some of his theses. These discussions survived in nominalist teaching in the 15th and 16th centuries, in writings of Nicholas of Cusa and Giordano Bruno (1548–1600), and eventually led to the atomic theory being used to explain chemical phenomena in the 17th century.

Concerning the problem of void, which arose partly out of the discussion of whether there were plural worlds--for if there were what lay between them?—such writers at the end of the 13th and beginning of the 14th century as Richard of Middleton (or Mediavilla, *fl.c.* 1294) and Walter Burley (1275–1344) went so far as to say that it was a contradiction of God's infinite power to say that He could not maintain an actual void. Nicholas of Autrecourt went further and affirmed the probable existence of a vacuum: 'There is something in which no body exists, but in which some body can exist,' he said in a passage published by J. R. O'Donnell in *Mediæval Studies* (1939, vol. 1, p. 218). Most writers accepted Aristotle's arguments and rejected an actually existing void (see Vol. I, p. 69), though they might accept Roger Bacon's description of void as a mathematical abstraction. 'In a vacuum nature does not exist,' he said in the *Opus Majus,* part 5, part 1, distinction 9, chapter 2.

> For vacuum rightly conceived of is merely a mathematical quantity extended in the three dimensions, existing *per se* without heat and cold, soft and hard, rare and dense, and without any natural quality, merely occupying space, as the philosophers maintained before Aristotle, not only within the heavens, but beyond.

Some of the physical arguments against the existence of void were taken from such ancient Greeks as Hero and Philo, whose experiments with the candle and the water clock or clepsydra were known to several writers, in particular Albertus Magnus, Pierre d'Auvergne (d. 1304), Jean Buridan (d. probably in 1358) and Marsilius of Inghen (d. 1396). Some of these writers also mentioned

another experiment in which water was shown to mount in a J-tube when air was sucked out of the long arm with the short arm under water. Another experiment was made with a water clock, with which it was shown that water would not run out of the holes in the bottom when the hole at the top was closed with the finger. This was contrary to the natural motion of water downwards and Albertus Magnus explained this as due to the impossibility of void, which meant that water could not run out unless air could enter and maintain contact with it. Roger Bacon was not satisfied with such a negative explanation. He held that the final cause of the phenomenon was the order of nature, which did not admit void, but the efficient cause was a positive 'force of universal nature,' an adaptation of the 'common corporeity' of Avicebron (see Vol. I, p. 73), which pressed on the water and held it up. This was similar to the explanation already given by Adelard of Bath. Giles of Rome later substituted another positive force, *tractatus a vacuo* or suction by a vacuum, a universal attraction which kept bodies in contact and prevented discontinuity. The same force, he held, caused the magnet to attract iron. Another 14th-century writer, John of Dumbleton (*fl.c.* 1331–49), said that to maintain contact celestial bodies would, if necessary, abandon their natural circular motions as particular bodies and follow their universal nature or 'corporeity,' even though this involved an unnatural rectilinear movement. In the 15th and 16th centuries, Roger Bacon's full theory was forgotten in Paris and condensed into the 'nature abhors a vacuum' that provoked the sarcasms of Torricelli and Pascal.

The possibility of both infinite addition and infinite division of magnitude led to interesting discussions on the logical basis of mathematics. It was asserted by Richard of Middleton and later by Ockham that no limit could be assigned to the size of the universe and that it was potentially infinite (see Vol. I, p. 73). It was not actually infinite, for no sensible body could be actually infinite. Richard of Middleton tried to show also that this last conclusion was incompatible with Aristotle's doctrine of the eternity of the universe, which Albertus Magnus and Thomas Aquinas had

said could be neither proved nor disproved by reason but must be denied from revelation. Richard said that as indestructible human souls were continually being generated, if the universe had existed from eternity there would now be an infinite multitude of such beings. An actually infinite multitude could not exist, therefore the universe had not existed from eternity. The whole discussion led to an examination of the meaning of infinity. The development of the geometrical paradoxes that would arise from the categorical assertion of an actually existing infinity, such as in Albert of Saxony's discussion of whether there could be an infinite spiral line on a finite body, led Gregory of Rimini (1344) to try to give precise signification to the words 'whole,' 'part.' 'greater,' 'less.' He pointed out that they had a different meaning when referring to finite and infinite magnitudes, and that 'infinity' had a different signification according to whether it was taken in a distributive or collective sense. This problem was discussed in the *Centiloquium Theologicum* formerly attributed to Ockham but of uncertain authorship. Conclusion 17, C shows that the author had achieved a logical subtlety which was to be recovered only in the 19th and 20th centuries in the mathematical logic of Cantor, Dedekind and Russell.

> There is no objection to the part being equal to its whole, or not being less, because this is found, not . . . only intensively but also extensively . . . for in the whole universe there are no more parts than in one bean, because in a bean there is an infinite number of parts.

These discussions of infinity and other problems, such as the maximum resistance a force could, and the minimum it could not overcome, laid the logical basis of the infinitesimal calculus. Medieval mathematics was limited in range and it was only when humanists had drawn attention to Greek mathematics, and especially to Archimedes, that the mathematical developments which actually took place in the 17th century became a possibility.

Associated with the problem of infinite magnitude was that of plural worlds. In 1277 the Bishop of Paris, Étienne

Tempier, condemned the proposition that it was impossible for God to create more than one universe. The problem was usually discussed in connection with gravity and the natural place of the elements (see Vol. I, pp. 76, 129).

In his *De Cælo* (book 1, chapter 8) Aristotle had briefly considered the possibility of a mechanical explanation of gravitation by external forces either pulling or pushing bodies, but he rejected this on the grounds that it was made unnecessary by the whole conception that the movements of gravity and levity were the spontaneous movements of a 'nature' towards its natural place (cf Vol. I, pp. 69–70, below, p. 47 *et seq.*). It was to this view that Averroës lent his authority, making gravity an *intrinsic* tendency belonging to the 'nature' or 'form' of a body and thus causing its movement. This conception of gravity and levity as intrinsic properties causing natural movement became the normal one in the 13th century, accepted for example by Albertus Magnus and Thomas Aquinas, although opinion differed as to the precise manner in which the 'form' caused a body to move.

But already in the 13th century there were natural philosophers who held that, over and above the natural spontaneity of the form and the final causality of the natural place, it was necessary to look for some further efficient causality of gravitation. Some writers conceived this as an *external* cause. Bonaventura and Richard of Middleton, for example, suggested that an attracting force (*virtus loci attrahentis*) should be attributed to natural place and an expelling force to unnatural place. Roger Bacon developed a complete 'field' theory to account for gravitation (cf. Vol. I, pp. 73, 100–6, below p. 61). He proposed that the natural place exercised not only final causality but also efficient causality through a *virtus immaterialis*, an immaterial power coming from the heavenly bodies and filling all space. Gravity and levity were diffused immaterial forces which, although derived from 'celestial virtue,' produced their effects by being concentrated more intensely in various natural places. This explanation is to be found also in the *Summa Philosophiæ* of pseudo-Grosseteste.

An even more extreme form of this explanation by external forces seems to have been put forward by some 14th-century writers who conceived natural place as a total efficient cause of gravitation. For example Buridan in his *Quæstiones de Cælo et Mundo* (book 2, question 12) mentions the opinion of 'certain people' (*aliqui*) who 'say that place is the motive cause of the heavy body by means of attraction, just as a magnet attracts iron.' He attacked this opinion on the grounds of experience. Since heavy bodies accelerate as they fall, he said, there must be an increase in the motive force commensurate with the increase in velocity (cf. Vol. I, pp. 76, 114–15, below, p. 66 *et seq.*). Those who hold that the motive force is attraction by the natural place must therefore suppose that this is greater near the natural place than farther off, as is the case with the magnet. But if two stones are dropped from a tower, one from the top and the other from lower down, the first has a much greater velocity than the second when both have reached, for example, a point a foot from the ground. Hence it is not simply nearness to the natural place that determines velocity, but, whatever the cause is, velocity depends on the length of the fall. 'Nor is it similar to the magnet and the iron,' he concluded, 'because if iron is near a magnet, it immediately starts moving more quickly than if it were farther removed: but this is not the case of heavy bodies with respect to their natural place.'[8]

A further objection to the natural place exerting any kind of force, any *vis trahens* on the body moving towards it, was made by Albert of Saxony (*c.* 1316–90). He pointed out that to such a force a heavier body would offer a greater resistance than a lighter body and so should fall more slowly than a lighter body, which was contrary to experience.

These arguments are a good example of the extreme difficulty which the dynamical problems whose solutions we now take for granted presented to those who first attacked them.

All these writers accepted the principle that action at

[8] In fact both magnetism and gravity give bodies an acceleration inversely proportional to the square of the distance.

a distance simply speaking was impossible, and those who proposed the analogy of the magnet usually had in mind the explanation of its action given by Averroës (see Vol. I, p. 122). According to this theory the force that moved the iron was a quality induced in it by the *species magnetica* that went out from the magnet through the medium and altered the iron, thus giving it the power *to move itself*. Thus was preserved the essential principle of Aristotelian dynamics, that the motive power must accompany the moving body.

An exception was William of Ockham. Arguing that intermediate 'species' and agents postulated simply to avoid having to accept action at a distance were not necessary to 'save the appearances,' he boldly declared that there was no objection to action at a distance as such. The sun in illuminating the earth acted at a distance immediately. The magnet, he asserted in his *Commentary on the Sentences* (book 2, question 18), 'pulls [the iron] immediately and not by means of a power existing in some way in the medium or in the iron; therefore the lodestone acts at a distance immediately and not through a medium.' As for the general principle that the motive power must accompany the moving body, Ockham's attack on the whole contemporary conception of motion altogether denied this as a premiss for dynamical explanations (see below, pp. 62–66).

At least one other 14th-century writer, John Baconthorpe, followed Ockham in accepting the possibility of action at a distance, asserting, as Dr. Maier quotes him in her book, *An der Grenze von Scholastik und Naturwissenschaft* (p. 176, note), that the magnet 'attracts the iron effectively.' But the common opinion on gravitation in the 14th century, as in the 13th, rejected both action at a distance and external forces of any kind and took Aristotle's and Averroës' view of it as an intrinsic tendency. This was the view, for example, of Jean de Jandun, Walter Burley, Buridan, Albert of Saxony, and Marsilius of Inghen. The attempt by Buridan and others to give quantitative precision to this intrinsic cause of motion led to the most

interesting dynamical theorising before Galileo (see below, pp. 66 *et seq.*, 152 *et seq.*).

The question then arose, what was the natural place of an element, for example earth, at which it came to rest? In discussing this problem Albert of Saxony (c. 1316–90) distinguished between the centre of volume and the centre of gravity. The weight of each piece of matter was concentrated at its centre of gravity and earth was in its natural place when its centre of gravity was at the centre of the universe. The natural place of water was in a sphere round the earth, so that it exerted no pressure on the surface of the earth which it covered.

Although Aristotelians like Buridan and Albert of Saxony rejected the explanation of gravity by external forces, the Aristotelian explanation did not remain alone in the field. With the revival of Platonism, especially in the 15th century, an argument for the existence of plural worlds was found in the conception of gravity of the Pythagoreans and Plato.

> Heraclides of Pontus and the Pythagoreans maintain that each of the stars constitutes a world, that it consists of an earth surrounded by air and that the whole is swimming in illimitable ether,

the 5th-century A.D. Greek writer Joannes Stobæus had said in his *Eclogarum Physicorum*, chapter 24. The theory of gravity derived from the *Timæus* was that the natural movement of a body was to rejoin the element to which it belonged, in whichever world it was, while violent movement had the opposite effect (see Vol. I, p. 30). This explanation of gravity as the tendency of all similar bodies to congregate, as *inclinatio ad simile*, was generally adopted by those who rejected Aristotle's conception of absolute space. The Aristotelian objection that if there were plural worlds there would be no natural place thus lost its point. Matter would simply tend to move towards the world nearest it. This theory was mentioned by Jean Buridan, himself a critic of Aristotle's absolute space although not of course of his natural place. It was adopted by Nicole Oresme (see below, pp. 65, 71–84) and later by the leading 15th-

century Platonist, Nicholas of Cusa, who said that gravitation was a local phenomenon and each star a centre of attraction capable of keeping together its parts. Nicholas of Cusa also believed that each star had its inhabitants, as the earth did. Albert of Saxony had retained the essential structure of the Aristotelian universe; Ockham, though he held, like Avicebron, that the matter of elementary and celestial bodies was the same, said that only God could corrupt the celestial substance. Nicholas of Cusa said that there was absolutely no distinction between celestial and sublunary matter and that since the universe, while not actually infinite, had no boundaries, neither the earth nor any other body could be its centre. It had no centre. Each star, of which our earth was one, consisted of the four elements arranged concentrically round a central earth and each was separately suspended in illimitable space by the exact balance of its light and heavy elements.

(3)   DYNAMICS — TERRESTRIAL AND CELESTIAL

Aristotle's dynamics involved several propositions all of which came to be criticised in the later Middle Ages. In the first place, there was Aristotle's conception of local motion, like all kinds of change, as a process by which the potentialities of any body to movement were made actual by a motive agent (see Vol. I, pp. 69–70, 76, 114–15). In natural motion this agent was an intrinsic principle, acting either as an efficient cause, for example the 'soul' in living things (cf. Vol. I, pp. 139–40), or as a principle producing characteristic spontaneous motion in a particular environment, as in the motion of bodies towards their 'natural place.' Each of the celestial spheres was also moved by a 'soul,' which became with later writers an 'Intelligence' that pushed the sphere round. In unnatural or forced 'violent' motion, the agent was always an external mover which accompanied the moving body and imposed its alien form of movement on it. But whether the motion was produced by the natural activity of the 'nature' or 'form' or was im-

posed by an external agent, the essential principle was preserved: 'Everything that is moved must be moved by something.' If the cause ceased, so did the effect. Basic to the whole conception of natural motion was that it proceeded towards an end, a goal, for example the earth as the goal of a naturally falling stone. Unnatural motion was the imposition of a motion alien to the natural goal, and such motion continued only so long as the external agent remained in contact with the body moved. Aristotle held further that the velocity of a moving body was directly proportional to the motive power and inversely proportional to the resistance of the medium in which movement took place. This gave the law,

$$\text{velocity } (v) \propto \frac{\text{motive power } (p)}{\text{resistance } (r)}.$$

It was an important limitation, coming from the Greek conception of proportion and from Aristotle's vague formulation, that Aristotle himself did not in fact express his 'law' in the manner in which, for convenience, it is written in the preceding line. According to the Greek conception, a magnitude could result only from a 'true' proportion, that is from a ratio between 'like' quantities, for example between two distances or two times. A ratio between two 'unlike' quantities such as distance (s) and time (t) would thus not have been considered as a magnitude, so that the Greeks did not in fact give a metric definition of velocity as a magnitude representing a ratio between space and time, i.e. $v = k \frac{s}{t}$. Such a metric definition was one of the achievements of the 14th-century scholastic mathematicians. Aristotle himself could express the relation of velocity to power and resistance only by taking the problem in separate stages. Thus $\frac{s_1}{s_2} = \frac{t_1}{t_2}$, i.e. speed is uniform, when $p_1 = p_2$ and $r_1 = r_2$; $\frac{s_1}{s_2} = \frac{p_1}{p_2}$ when $t_1 = t_2$ and $r_1 = r_2$; $\frac{s_1}{s_2} = \frac{r_2}{r_1}$ and when $t_1 = t_2$ and $p_1 = p_2$.

Aristotle's 'law' expressed his belief that any increase in velocity in a given medium could be produced only by an

increase in motive power. It also followed from the 'law' that in a void bodies would fall with instantaneous velocity; as he regarded this conclusion as absurd he used it as an argument against the possibility of a void. He held that in a given medium bodies of various materials but of the same shape and size fell with velocities proportional to their various weights.

This conception and classification of motion was based on direct observation and it was confirmed by many everyday phenomena. But three phenomena presented difficulties that were ultimately to prove fatal to the mathematical formulation drawn from Aristotle's account. First, according to Aristotle's 'law,' there should be a finite velocity (v) with any finite values of power (p) and resistance (r), yet in fact if the power were smaller than the resistance it might fail to move the body at all. Aristotle himself recognised this and made reservations for his law, for example in the case of a man trying to move a heavy weight and not succeeding.

Secondly, what was the source of the increase in motive power required to produce the acceleration of freely falling bodies? He had seen that bodies falling vertically in air accelerated steadily, and he thought that this was because the body moved more quickly as it got nearer to its natural place in the universe as the goal and fulfilment of its natural motion.

Thirdly, what was the motive power that kept a projectile in motion after it had left the agent of projection? If the upward movement of a stone was not due to the stone itself but to the hand that threw it, what was responsible for its continued movement after it ceased to be in contact with the hand? What kept an arrow in flight after it had left the bowstring? Aristotle himself in the *Physics* (book 8) proposed this problem and discussed two solutions, Plato's and his own. In the *Timæus* Plato had given to bodies only one proper motion, that towards their proper place in space forming the receptacle of all things, and this motion he explained by the geometrical shape of the elementary bodies and the shaking of the receptacle by the World Soul. All other movements he attributed to col-

lision and mutual replacement, *antiperistasis:* a projectile, for example, at the moment of discharge compressed the air in front of it, which then circulated to the rear of the projectile and pushed it forward, and so on in a vortex. Aristotle's objection to this explanation was that unless the original mover gave, to what it moved, not only motion but also the power to be a mover itself, the motion would cease. He therefore proposed that the bowstring or hand communicated a certain quality or 'power of being a movent' (as he said in book 8, chapter 10 of the *Physics*, 267a 4) to the air in contact with it, that this transmitted the impulse to the next layer of air, and so on, thus keeping the arrow in motion until the power gradually died away. This power, he said, came from the fact that air (and water), being intermediate elements, were heavy or light, depending on their actual environment. The air could thus move a projectile upwards from its own natural motion. If actual space were a void, he argued in book 4 of the *Physics*, not even forced motion would be possible: a projectile would not be able to move in void space.

As seen in the light of the classical mechanics completed in the 17th century, the notorious defect of Aristotle's mechanics was its failure to deal adequately with *acceleration* as distinct from velocity. From the point of view of these later conceptions, his fundamental difficulty arose from the fact that by analysing motion entirely in terms of velocities continuing over a period of time, he was unable to deal with *initial* velocity, or with the force required to *start* a body moving. His idea of force or power is restricted to that causing motions continuing over a period of time. All the difficulties found in his treatment were finally overcome when motion was analysed in terms of velocity *at an instant.* Using this conception, Newton was able to show that the same initial force that started a body moving must, if it continued to act, produce not just continued velocity but the same constant change in velocity, that is, constant acceleration. The moves towards clarity in these problems that were made before Newton will be seen in the sequel.

Parts of Aristotle's dynamics had already been criticised in the Ancient World by members of other schools of

thought. The Greek atomists had considered it an axiom that all bodies of whatever weight would fall in a void with the same velocity, and that differences in the velocity of given bodies in a given medium, for instance air, were due to differences in the proportion of resistance to weight (see above, p. 37, note). The Alexandrian mechanicians and the Stoics had also admitted the possibility of void, but Philo had said that differences in velocity of fall were due to different 'weight-forces' (corresponding to different 'masses') and from this Hero drew the corollary that if two bodies of a given weight were fused, the speed of fall of the united body would be greater than that of each singly. The Christian Neoplatonist, John Philoponus of Alexandria, writing in the 6th century A.D., had also rejected both Aristotle's and the atomists' laws regarding falling bodies and maintained that in a void a body would fall with a finite velocity characteristic of its gravity, while in air this finite velocity was decreased in proportion to the resistance of the medium. The rotation of celestial spheres provided an example of a finite velocity that took place in the absence of resistance. Philoponus also pointed out that the velocities of bodies falling in air were not simply proportional to their weights, for when a heavy and a less heavy body were dropped from the same height, the difference between their times of fall was much smaller than that between their weights. Philoponus did accept Aristotle's theory for explaining the continuous acceleration of falling bodies, though this was not accepted by other late Greek physicists. Some of these put forward an adaptation of the Platonic conception of *antiperistasis*, according to which the falling body forced down the air which then drew the body after it and so on, natural gravity both receiving continuously increasing assistance from the traction of the air and continuously causing an increase in that assistance.

Philoponus seems to have been the first to show that the medium cannot be the cause of projectile motion. If it is really the air that carries the stone or the arrow along, why, he asked, must the hand touch the stone at all or the arrow be fitted to the bow? Why does not violent beating of the

air move the stone? Why can a heavy stone be thrown
farther than a very light one? Why do two bodies have to
collide to be deflected and not simply pass close to each
other through the air? These everyday observations, which
were to form the staple of criticism of Aristotle's dynamics
down to the time of Galileo himself, led Philoponus to
propose an alternative explanation of the 'forced' motion
of projectiles. Obviously the air did not produce the mo-
tion but resisted it. He put forward the original idea that
the instrument of projection imparted motive power not
to the air but to the projectile itself: 'a certain incorporeal
motive power must be given to the projectile through the
act of throwing,' he said in his commentary on Aristotle's
Physics (book 4, chapter 8). But this motive power, or
'energy' (*energeia*), was only borrowed and was decreased
by the natural tendencies of the body and by the resistance
of the medium, so that the projectile's unnatural motion
eventually came to an end.

Philoponus' theory has been claimed by some scholars,
notably by Duhem, as the origin of certain medieval con-
ceptions that have been supposed in turn to have given rise
to the modern conception of inertia, which was to be the
basis of the revolution in dynamics in the 17th century
(see below, p. 63, note). We will see on a later page that
this view of complete continuity may be questioned on the
grounds both of the actual historical derivation and of the
character of the conception of motion concerned. But the
theory that unnatural motion could be maintained by a
motive power imparted to the unnaturally moving body
itself was an important innovation and it was mentioned
by several writers before it reappeared as the theory of
impetus in the 14th century. Philoponus himself was at-
tacked by Simplicius (d. 549) in the 'Digressions against
John the Grammarian' which he appended to his own com-
mentary on the *Physics*. He objected specifically to Philo-
ponus' denial of the fundamental principle that whatever
is moved unnaturally must be moved by an external agent
in contact with it. His own explanation of projectile mo-
tion was a development of the *antiperistasis* theory: he
held that the projectile and the medium alternately acted

on each other until eventually the motive power became exhausted. At the same time he put forward an explanation of the acceleration of freely falling bodies by supposing that their weight increased as they approached the centre of the world.

The first Arabic writer known to have taken up Philoponus' theory was Avicenna, who defined the power imparted to the projectile, as S. Pines translates him in an important article in *Archeion* (1938, vol. 21, p. 301), as 'a quality by which the body pushes that which prevents it moving itself in any direction.' He called this also a 'borrowed power,' a quality given to the projectile by the projector as heat was given to water by a fire. Avicenna made two important modifications of the theory. First, whereas Philoponus had held that even in a void, if this were possible, the borrowed power would gradually disappear and the projectile's 'forced' motion cease, Avicenna argued that in the absence of any obstacle this power, and the 'forced' motion it produced, would persist indefinitely. Secondly, he tried to express the motive power quantitatively, saying in effect that bodies moved by a given power would travel with velocities inversely proportional to their weights, and that bodies moving with a given velocity would travel (against the resistance of the air) distances directly proportional to their weights. A further development of the theory was made by Avicenna's 12th-century follower Abu al-Barakat al-Baghdadi, who proposed an explanation of the acceleration of falling bodies by the accumulation of successive increments of power with successive increments of velocity.

The main points at issue between the Aristotelian conception of motions and this ultimately Neoplatonic conception, first expounded by Philoponus, were taken up by Averroës in a discussion that was to determine the main lines of the debate that began in the West in the 13th century. Philoponus had maintained that in all cases, in falling bodies and in projectiles, velocity was proportional only to motive power, and that the resistance of the medium merely reduced it from a definite finite velocity. This 'law of motion' was advocated by the 12th-century Spanish

Arab Ibn Badga, or Avempace as he was called in Latin, as an alternative to Aristotle's. It meant substituting for Aristotle's 'law of motion' the formula: velocity (v) = power (p) — resistance (r). Avempace argued that even in a void a body would move with finite velocity because, although there was no resistance, the body would still have to traverse *distance*. Like Philoponus he cited the motion of the celestial spheres as an example of finite velocity without resistance. In his commentary on Aristotle's *Physics* Averroës attacked not only Avempace's account of motion (which he thought was original) but the whole conception of 'natures' on which it was based. Avempace's mistake, he maintained, was to treat the 'nature' of a heavy body as if it were an entity distinct from the matter of the body, and as if the matter were moved by the 'form' acting as an efficient cause in the same way as an immaterial Intelligence moved its celestial sphere or the 'soul' caused the movements of a living organism. Averroës specifically objected to Avempace's assumption that the medium was an impediment to natural motion, for this would mean that all actual bodies moved unnaturally, since all do in fact move through corporeal media.

The natural point of departure for the scholastic commentators on Aristotle's *Physics* and *De Cælo* were the commentaries by Averroës that accompanied the most popular early Latin versions. Averroës' exposition and criticism of Avempace thus became the source of a major divergence in the attempts to formulate a law relating the velocities of natural motions. But it marked more than that. It has been claimed that it reflected a major cleavage in the conception of nature which runs through the whole history of philosophy.[9] Philoponus and Avempace had followed Plato in looking for the real natures and causes of phenomena not in immediate experience but in factors abstracted by reason from experience. It might be that all observed bodies do in fact move through a medium; the *law* of their motion was nevertheless to be sought, not in immediate experience, but by abstract analysis which dis-

covered the intelligible real world as an idealisation of which the multifarious diversity of the world of experience was the composite product and in a sense the 'appearance.' Against this Averroës identified the real world with the directly observable and the concrete, and looked for the law of motion close to the data of experience in all their immediate diversity.

The conclusion of Averroës' line of argument would be to attribute the abstract factors into which we analyse immediate experience to our ways of thinking rather than to the things thought of, to regard these factors as mere concepts or even names, not as discoveries of something real. This was the issue between the 'nominalists' and 'realists' in the Middle Ages and between the 'empiricists' and 'rationalists' in the 17th and 18th centuries. It represents a major difference not only in philosophy of nature but in scientific method. Certainly Averroës and his Western followers saw their close empiricism as a true expression of Aristotelian methods, whereas Avempace was described by Albertus Magnus and Aquinas as a Platonist, and Galileo was to claim his method of mathematical idealisation as a triumph for Plato over Aristotle. The methods applied on the different sides of the debate in the 13th and 14th centuries can be seen from these two points of view, although the positive contributions to the problem of motion by no means all came from one side.

In the 13th century it was mainly the philosophical issues that determined the terms of the discussion of motion, but this gave way in the 14th century to a greater attention to the mathematical and quantitative formulation of laws of motion. Attention began to turn from the 'why' to the 'how.' Practically without exception—the most significant was William of Ockham—the natural philosophers of this period based their discussions on the accepted Aristotelian principle that being in motion meant being moved by something. Differences of opinion concerned the nature of the moving power in the different cases and the quantitative relations between the different determinants of velocity.

The first scholastic philosopher to take up the debate between Averroës and Avempace was Albertus Magnus. He stood firm for Averroës, and in this he was followed by Giles of Rome and others, until in the 14th century Thomas Bradwardine produced an original version of the Aristotelian 'law' expressing the proportionality between velocity and power and resistance. Averroës had taken up Aristotle's own reservations about the law $v \propto p/r$, in the case where power failed to overcome resistance and produce any movement at all (see above, p. 48). He had tried to overcome this difficulty by saying that velocity followed the *excess* of power over resistance, and some 13th-century Latin writers supposed movement to arise only when p/r was greater than 1. Thomas Bradwardine, in his *Tractatus Proportionum* (1328), limited comparisons of the proportion of power to resistance to cases when this was so. He tried, in what seems to be one of the earliest attempts to use algebraic functions to describe motion, to show how the dependent variable v was related to the two independent variables p and r.

The formulation of the Aristotelian 'law of motion' metrically as a function, so that it became quantitatively refutable, was an achievement of the greatest importance, even though neither Bradwardine nor any of his contemporaries discovered an expression that fitted the facts or indeed applied any empirical quantitative tests. The first requirement was to give a metric definition of velocity as a magnitude representing a ratio between space and time. Aristotle had not only failed to do this, but his method of expression had not clearly distinguished the static analysis of the relationship between power (p), resistance (r) and distance (s), where time (t) is not considered, for example in dealing with the lifting of weights, from the kinematic-dynamic analysis where time is considered (cf. Vol. I, pp. 115–16). The first writer, at least in the West, to attempt a purely kinematic analysis of motion seems to have been Gerard of Brussels, whose important treatise *De Motu* was composed, according to Clagett, possibly between 1187 and 1260. It appears to have been associated in some way with the activities of Jordanus, and it shows the strong influence

of Euclid and Archimedes, making use of the latter's characteristic type of proof by *reductio ad absurdum* (or proof *per impossibile*) and method of exhaustion. Dealing with movements of rotation, Gerard took an approach that has become characteristic of modern kinematics, seeing as the basic objective of analysis the representation of non-uniform velocities by uniform velocities. Although he fell short of defining velocity as a ratio of unlike quantities, his analysis inevitably involved the concept of velocity, and he seems to have assumed that the speed of a motion can be assigned some number or quantity making it a magnitude like space or time. Bradwardine specifically discussed some of Gerard's propositions, and it seems probable that *De Motu* directed the attention of the Oxford mathematicians of the 14th century to the kinematic description of variable movements and to the metric definition of velocity required for their treatment (cf. below, p. 93 *et seq.*).

Using his metric formulation, Bradwardine was able to show that Aristotle's analysis and various other current formulæ, including Avempace's, did not fit the facts of *moving* bodies, as he understood them. He rejected them all because they did not satisfy his physical presuppositions or hold for all values. In their place he proposed an interpretation of Aristotle's law based on the theorem given in Campanus of Novara's commentary on Euclid's fifth book, in which it was proved that if $a/b = b/c$, then $a/c = (b/c)^2$. Bradwardine argued that Aristotle's law meant that if a given ratio p/r produced a velocity v, then the ratio that would double this velocity was not $2p/r$ but $(p/r)^2$, and the ratio that would halve it was $\sqrt{p/r}$. The exponential function by which he related these variables may be written, in modern terminology, $v = \log (p/r)$. Since the logarithm of $1/1$ is zero, the condition is satisfied that when force and resistance are equal, no motion results, and the formula gives a continuous gradual change in v as p/r approaches 1. Although Bradwardine's treatment of dynamics suffered from the serious defect (by no means unique in the period) that he did not test his 'law' by making measurements, his formulation of the problem in terms of an equation in which the complexity of the rela-

tions involved were recognised was an important contribution to the methods of mathematical physics. His shifting of the ground of the discussion of motion from 'why' to 'how' had an immediate and lasting influence. His equation was accepted by the Oxford mathematicians Heytesbury, Dumbleton and Richard Swineshead (see below, p. 93) and by Buridan, Albert of Saxony and Nicole Oresme, and down to the 16th century it was almost universally held to be the true Aristotelian 'law of motion.'

The earliest and most important critic of Aristotle's 'law of motion' from the point of view of Avempace was Thomas Aquinas. The main point at issue was whether a body would move with finite velocity in a void. In his commentary on the *Physics* Aquinas supported Avempace's argument that even without any resistance, all motion must take time because it traverses extended distance. Hence he accepted Avempace's 'law,' $v = p - r$. He was even prepared to accept Averroës' assertion that this would imply an 'element of violence' in all actual natural motions, for these *all started* from an unnatural place. Roger Bacon, Peter Olivi (1245/49–98), Duns Scotus and other 13th-century writers followed Aquinas in defending Avempace. In the 14th century his 'law' was generally rejected under the influence of Averroës and Bradwardine, but it found a supporter towards the end of the century in a certain Magister Claius. He held that heavy bodies would fall in a void faster than light bodies, but that none would have an infinite velocity. It was an expression for motion identical with Avempace's that Galileo was to use in his early work on dynamics at Pisa.

Associated with Avempace's quantitative analysis of motion, there were new attempts in the 13th century to explain the cause of the acceleration of freely falling bodies and of the continued velocity of projectiles. Clearly the medium could be of no assistance if these were considered *in vacuo*. It is a disputed point whether Aquinas himself accepted the theory that the original agent impressed on the projectile some kind of power, some *virtus impressa*, which acted as the *instrument* of its continued motion. Certainly he discussed this theory, but he also distin-

guished clearly between natural motive powers such as the intrinsic power of growth given by the father to the seed in reproduction, and the unnatural extrinsic power moving a projectile. The latter he seems in fact to have attributed to the medium. Olivi did propose an explanation of projectile motion by what he called, in his *Quæstiones in secundum librum Sententiarum,* 'violent impulses or inclinations given by the projector,' comparable with the natural impulses of heaviness and lightness. The context of Olivi's explanation was the problem of action at a distance in a discussion of causality in general. He cited projectile motion as an example of action caused not by direct contact, or by the medium, but by 'species' or 'similitudes' or 'impressions' impressed by the agent of projection on the projectile and moving it after separation from the thrower. In fact Olivi's explanation was an adaptation of the theory of the 'multiplication of species' of Grosseteste and Roger Bacon (cf. Vol. I, pp. 74, 99–100, above, p. 43 *et seq*). It was basically a Neoplatonic emanation, and essential to it was that it moved towards a goal.

The first scholastic natural philosopher to put forward a theory of 'impressed force' as an Aristotelian motive power, a *vis motrix* determined not by the goal but by the projecting agent, seems to have been an Italian follower of Duns Scotus, Franciscus de Marchia. In his commentary on the *Sentences,* written about 1320 in Paris, Marchia followed Aquinas in discussing the problem of instrumental causality. The context of the problem, moving by analogy with ease from theology to physics, is characteristic of much scholastic natural philosophy. In inquiring whether any power to produce grace resided in the sacraments themselves or came only direct from God, Marchia raised the question of projectile motion in order to show that both in the sacraments and in projectiles there was a certain residual power that was capable of producing effects. Rejecting Aristotle's theory that projectile motion was caused by the air, he concluded that it must be explained, as translated from the passage quoted by Dr. Maier in her *Zwei Grundprobleme der Scholastischen Naturphilosophie* (p. 174), 'by the motion or impulse of a power left behind

(*virtus derelicta*) in the stone by the primary mover,' that is, by the hand or the bowstring. Marchia was careful to point out that this power was not innate or permanent. It was an accidental quality, which was extrinsic and violent, and being opposed to the natural inclinations of the body it endured only for a certain time. The motive power of a projectile was, he said, a 'form' that was neither wholly permanent, like whiteness or the heat of fire, nor wholly transient (*fluens, successiva*) like the process of heating or of moving, but something intermediate which endured for a limited time.

The existence of a similar 'law of motion' and similar conceptions of motive power in the writings of Philoponus and Avempace and of the scholastics of the 13th and 14th centuries has naturally led historians to look for a possible historical connection between them. Certainly nearly all these writers belong to the Neoplatonic tradition, but no actual documentary derivation has been traced. So far as is known, Philoponus' own writings were not known in the Middle Ages. Direct medieval knowledge of his views seems to have been largely limited to the incomplete and not very clear presentation of his position by Simplicius, whose commentary on the *Physics* was translated into Latin in the 13th century. Avicenna's discussion of projectile motion and 'impressed power' does not occur in the part of his commentary that was translated into Latin under the name *Sufficientia Physicorum*, which contains only the first four books (cf. Vol. I, p. 41). Alpetragius is known to have been strongly influenced by a disciple of Avempace, Ibn Tofail, and the Latin translation of Alpetragius' work made in 1528 and published in Venice in 1531 as *Theorica Planetarum* gave a clear account of Philoponus' theory, though not under his name. But in the medieval translation, made by Michael Scot in 1217 with the name *Liber Astronomiæ*, the theory is abridged out of existence in the passage concerned. So far as the evidence goes, Dr. Maier has concluded that the theory of 'impressed power,' and that of *impetus* which succeeded it in the 14th century, were developed independently by the scholastics, mainly

through their discussions of instrumental causality in re-production and in the sacraments.

Not all natural philosophers in the 13th and 14th centuries accepted this view of the cause of projectile motion, and there were many, for example Giles of Rome, Richard of Middleton, Walter Burley and Jean de Jandun, who continued to accept Aristotle's explanation, however unsatisfactory, because they were even more dissatisfied with the alternatives. They objected both to mediated action at a distance by the 'multiplication of species' and to 'impressed power' as being equally impossible. The author of *De Ratione Ponderis*, of the school of Jordanus Nemorarius (see Vol. I, pp. 118–19), held that the air caused both the continued velocity and a supposed initial acceleration of projectiles; in the 16th century this theory was still partly accepted even by such physicists as Leonardo da Vinci, Cardano and Tartaglia.

To explain the acceleration of freely falling bodies, many natural philosophers continued to follow either Aristotle or the theory using the air and *antiperistasis*. An original account of falling bodies was put forward by Roger Bacon. He supposed that each particle in a heavy body naturally tended to fall by the shortest route towards the centre of the universe, but that each tended to be displaced from this straight path by the parti, 'es lateral to it. The resulting mutual interference by the different particles acted as an internal resistance which would make movement take time even in a void, where there was no external resistance, and so Aristotle's argument that it would be instantaneous did not hold.

As to the nature of the 'form' that was the physical cause of movement, that is, the nature of the motive power that all these theories presupposed as necessary for the state of being in motion, at least two different views were hotly argued in the 14th century. The first view was that usually associated with Duns Scotus, namely, the theory that motion was a 'fluent form' or *forma fluens*. According to this theory, motion was an incessant flow in which it was impossible to divide or isolate a state, and a moving body was successively determined by a form distinct at once

from the moving body itself and from the place or space through which it moved. This theory was held by Jean Buridan and Albert of Saxony. The second view was that motion was a 'flux of form' or *fluxus formæ*, according to which motion was a continuous series of distinguishable states. One form of this theory was held by Gregory of Rimini, who identified motion with the space acquired during the movement, and said that during motion the moving body acquired from instant to instant a series of distinct attributes of place.

A third conception of motion, starting from a radically different point of view, was put forward by Ockham. One of the principal objects of Ockham's logical inquiries was to define the criteria by which a thing could be said to exist (cf. above, pp. 29–33). Nothing really existed, he held, except what he called *res absolutæ* or *res permanentes*, individual things, substances determined by observable qualities. 'Apart from *res absolutæ*, that is substances and qualities,' he said in the *Summa Totius Logicæ*, part 1, chapter 49, 'no thing is imaginable either in actuality or in potentiality.' Words like 'time' and 'motion' did not designate *res absolutæ* but relations between *res absolutæ*. They designated what Ockham called *res respectivæ*, without real existence. It is this careful analysis of the references of terms that is so striking a feature of Ockham's work, and it was through this that he and the other 'terminists' did so much to clarify many issues in 14th-century philosophy. As he said in his *Summulæ in Libros Physicorum*, book 3, chapter 7: 'If we sought precision by using words like "mover," "moved," "movable" and the like, instead of words like "motion," "mobility" and others of the same kind, which according to the form of language and to the opinion of many do not seem to stand for permanent things, many difficulties and doubts would be excluded. But now, because of these, it seems as if motion were some independent thing quite distinct from the permanent things.'

Applying these distinctions to the problems of dynamics, Ockham rejected altogether Aristotle's basic principle that local motion was a realised potentiality. He defined motion

as the successive existence, without intermediate rest, of a
continuous identity existing in different places, and for
him movement itself was a concept having no reality apart
from the moving bodies that could be perceived. It was
unnecessary to postulate any inhering form to cause the
movement, any real entity distinct from the moving body,
any flux or flow. All that need be said was that from instant
to instant a moving body had a different spatial relation-
ship with some other body. Every new effect required a
cause, but motion was not a new effect, since it was nothing
except that the body existed successively in different places.
Ockham therefore rejected all three current explanations
of the cause of projectile motion, the impulse of the air,
action at a distance mediated by 'species,' and 'impressed
power' given to the projectile itself (cf. above, p. 43). 'I
say therefore,' he said in his *Commentary on the Sentences*,
book 2, question 26, M, 'that that which moves (*ipsum
movens*) in motion of this kind, after the separation of
the moving body from the original projector, is the body
moved by itself (*ipsum motum secundum se*) and not by
any power in it or relative to it (*virtus absoluta in eo vel
respectiva*), for it is impossible to distinguish between that
which does the moving and that which is moved (*movens
et motum est penitus indistinctum*). If you say that a new
effect has some cause and that local motion is a new effect,
I say that local motion is not a new effect in the sense of
a real effect . . . , because it is nothing else but the fact
that the moving body is in different parts of space in such
a manner that it is not in any one part, since two contra-
dictories cannot both be true . . . Though any particular
part of space which the moving body traverses is new with
respect to the moving body, seeing that the body now moves
through those parts and previously was not doing so, yet
that part is not new really speaking. . . . It would indeed
be astonishing if my hand were to cause some power in
the stone by the mere fact that through local motion it
came into contact with the stone.'[10]

This conception he amplified with an application of the principle of economy in the so-called *Tractatus de Successivis* edited by Boehner, asserting in part 1 (p. 45):

> Motion is not such a thing wholly distinct in itself from the permanent body, because it is futile to use more entities when it is possible to use fewer . . . That without such an additional thing we can save motion, and everything that is said about motion, is made clear by considering the separate parts of motion. For it is clear that local motion is to be conceived as follows: positing that the body is in one place and later in another place, thus proceeding without any rest or any intermediate thing other than the body itself and the agent itself which moves, we have local motion truly. Therefore it is futile to postulate such other things.

The same applied, he said, to change in quality and to growth and decrease (cf. Vol. 1, p. 71). He continued in part 3 (pp. 121–22):

> It is clear how 'now before' and 'now after' are to be assigned, treating 'now' first: this part of the moving body is now in this position, and later it is true to say that now it is in another position, and so on. And so it is clear that 'now' does not signify anything distinct but always signifies the moving body itself which remains the same in itself, so that it neither acquires anything new nor loses anything existing in it. But the moving body does not remain always the same with respect to its surroundings, and so it is possible to assign 'before and after,' that is, to say: 'this body is now at A and not at B,' and later it will be true to say: 'this body is now at B and not at A,' so that contradictories are successively made true.

It has been claimed by some historians that by rejecting the basic Aristotelian principle expressed by the phrase *Omne quod movetur ab alio movetur*, Ockham took the first step towards the principle of inertia[11] which was to

[11] According to the principle of inertia a body will remain in a state of rest or of motion with uniform velocity in a straight line

revolutionise physics in the 17th century. Certainly by asserting the possibility of motion under the action of *no* motive power, a possibility formally excluded by the Aristotelian principle, Ockham opened the way to the principle of inertia and to the 17th-century definition of force as that which *alters* the state of rest or of uniform velocity, in other words, that which produces acceleration. The relevance of Ockham's conception of motion to 17th-century ideas becomes even more suggestive when taken in conjunction with the ideas of some other 14th-century writers. Nicholas Autrecourt, for example, related it to his conception of the atomic nature of a continuum and of time. Marsilius of Inghen, though himself rejecting Ockham's conception of motion, discussed it in connection with the conception of infinite space, an idea closely related to the 'geometrisation of space' in the 17th century. Nicole Oresme (d. 1382), though he retained the *forma fluens* to explain motion, put forward the idea that absolute motion could be defined only by reference to an immovable infinite space, placed beyond the fixed stars and identified with the infinity of God. From such passages Newton does not seem so far away, both as a physicist and as a natural theologian.

But the relation, both logical and historical, of Ockham's conception of motion to the principle of inertia is by no means straightforward. If we are tempted to read his statements in the light of Descartes' similar assertion that he made no distinction between motion and body in motion, we must also remember that for Descartes and for Newton the change in spatial relationships in passing from a state of rest to a state of motion *was* a new effect. It was an effect that required for its production not only a cause, but a precisely determined one. From Ockham's concep-

---

unless acted on by a force. This conception was the basis of Newton's mechanics. For Newton uniform rectilinear motion was a condition or state of the body equivalent to rest and no force was required to maintain such a state. The principle of inertia was thus directly contrary to Aristotle's principle according to which motion was not a state but a process and a moving body would cease to move unless continually acted on by a moving force.

tion of motion it is not in fact possible to deduce some
of the essential properties of the conservation of speed and
direction implied by the modern principle of inertia. Yet
Ockham had not overlooked the dynamical aspects of mo-
tion. In his *Expositio super Libros Physicorum*, when dis-
cussing the controversy between the supporters of Aver-
roës and of Avempace, he defended Aquinas for asserting
that where there was no resistance motion would take time,
the length of time depending on the distance. But where
there was a material resistance, he said that the time would
depend on the proportion of the motive power to the re-
sistance. In this way he distinguished what we would now
call the kinematic measure of velocity from the dynamical
measure of the motive power or force in terms of the work
done. The confusion of these measures is another example
of the difficulty with which the (to us) apparently most
elementary mechanical concepts were grasped, a difficulty
which even the entire 17th century did not wholly over-
come. When Bradwardine rejected Avempace's 'law of mo-
tion,' he made use of arguments similar to Ockham's, and
it is difficult not to see a connection in the common shift
of the problem from the 'why' to the 'how' which Ockham
made as a logician and Bradwardine as a mathematical
physicist.

In the event it was not Ockham who produced the most
significant and influential new dynamical theory in the 14th
century, but a physicist whose outlook was profoundly op-
posed to that of the 'terminists,' Jean Buridan, twice Rector
of the University of Paris between 1328 and 1340. Buridan
discussed the classical problems of motion in his *Quæs-
tiones super Octo Libros Physicorum* and in his *Quæs-
tiones de Cælo et Mundo*. To the existing criticisms of the
Platonic and the Aristotelian theories of projectile motion,
he added that the air could not account for the rotational
motion of a grindstone or a disc, for the motion contin-
ued even when a covering was placed close to the bodies,
thus cutting off the air. He rejected likewise the explana-
tion of the acceleration of freely falling bodies by their at-
traction to the natural place, because he maintained that
the mover must accompany the body moved (cf. above,

p. 45 *et seq.*). The theory of *impetus* by means of which he explained the various phenomena of persistent and accelerated motion was based, like the earlier theory of *virtus impressa*, on Aristotle's principles that all motion requires a motive power and that the cause must be commensurate with the effect. In this sense the theory of *impetus* was the historical conclusion of a line of development within Aristotelian physics, rather than the beginning of a new dynamics of inertia, of which, since it lay in the future, Buridan himself naturally knew nothing. But, under the influence of Bradwardine, Buridan formulated his theory with much greater quantitative precision than any of its predecessors. It is this aspect of some of his essential definitions that looks to the future.

Since other explanations of the persistence of motion of a body after separation from the original mover failed, Buridan concluded that the mover must impress on the body itself a certain *impetus*, a motive power by which it continued to move until affected by the action of independent forces. In projectiles this *impetus* was gradually reduced by air resistance and natural gravity downwards; in freely falling bodies it was gradually increased by natural gravity acting as an accelerating force which added successive increments of *impetus*, or 'accidental gravity,' to that already acquired. The measure of the *impetus* of a body was the quantity of matter in it multiplied by its velocity.

'Therefore it seems to me,' wrote Buridan in his *Quæstiones super Octo Libros Physicorum,* book 8, question 12, 'that we must conclude that a mover, in moving a body, impresses on it a certain *impetus*, a certain power capable of moving this body in the direction in which the mover set it going, whether upwards, downwards, sideways or in a circle. By the same amount that the mover moves the same body swiftly, by that amount is the *impetus* that is impressed on it more powerful. It is by this *impetus* that the stone is moved after the thrower ceases to move it; but because of the resistance of the air and also because of the gravity of the stone, which inclines it to move in a direction opposite to that towards which the *impetus* tends

to move it, this *impetus* is continually weakened. There-
fore the movement of the stone will become continually
slower, and at length the *impetus* is so diminished or de-
stroyed that the gravity of the stone prevails over it and
moves the stone down towards its natural place.

'One can, I think, accept this explanation because the
other explanations do not appear to be true, whereas all
the phenomena accord with this one.

'For if it is asked why I can throw a stone farther than
a feather and a piece of iron or lead suited to the hand
farther than a piece of wood of the same size, I say that
the cause of this is that the reception of all forms and
natural dispositions is in matter and by reason of matter.
Hence, the greater quantity of matter a body contains, the
more *impetus* it can receive and the greater the intensity
with which it can receive it. Now in a dense, heavy body
there is, other things being equal, more *materia prima* than
in a rare, light body.[12] Therefore a dense, heavy body re-
ceives more of this *impetus* and receives it with more in-
tensity [than a rare, light body]. In the same way a certain
quantity of iron can receive more heat than an equal quan-
tity of wood or water. A feather receives so feeble an im-
petus that it is soon destroyed by the resistance of the air
and, similarly, if one projects with equal velocity a light
piece of wood and a heavy piece of iron of the same size
and shape, the piece of iron will go farther because the
impetus impressed on it is more intense, and this does
not decay as fast as the weaker *impetus*. It is for the same
cause that it is more difficult to stop a big mill wheel,
moved rapidly, than a smaller wheel: there is in the big

---

[12] Buridan's *materia prima* was, like that in the *Timæus*, al-
ready extended with dimensions. Quantity of matter was then
proportional to volume and density. Duhem (*Études sur Léonard
de Vinci*, 3e série, 1913, pp. 46–49) suggests that he approached
the notion of density through that of specific weight, to which it
was proportional. The Greek pseudo-Archimedean *Liber Archi-
medis de ponderibus* defined specific weight and showed how to
compare the specific weights of different bodies by the hydrostatic
balance or areometer. This work was well known in the 13th and
14th centuries.

wheel, other things being equal, more *impetus* than in the small. In virtue of the same cause you can throw a stone of one pound or half a pound farther than the thousandth part of this stone: in this thousandth part the *impetus* is so small that it is all soon overcome by the resistance of the air.

'This seems to me to be also the cause on account of which the natural fall of heavy bodies goes on continually accelerating. At the beginning of this fall, gravity alone moved the body: it fell then more slowly; but, in moving, this gravity impressed on the heavy body an *impetus*, which *impetus* moves the body at the same time as gravity. The movement therefore becomes more rapid, and by the amount that it is made more rapid, so the more intense the *inpetus* becomes. It is thus evident that the movement will go on accelerating continually.

'Anyone who wants to jump far draws back a long way so that he can run faster and so acquire an *impetus* which, during the jump, carries him a long distance. Moreover, while he runs and jumps he does not feel that the air moves him, but he feels the air in front of him resist with force.

'One does not find in the Bible that there are Intelligences charged to communicate to the celestial spheres their proper motions; it is permissible then to show that it is not necessary to suppose the existence of such Intelligences. One could say, in fact, that God, when he created the universe, set each of the celestial spheres in motion as it pleased him, impressing on each of them an *impetus* which has moved it ever since. God has therefore no longer to move these spheres, except in exerting a general influence similar to that by which he gives his concurrence to all phenomena. Thus he could rest on the seventh day from the work he had achieved, confiding to created things their mutual causes and effects. These *impetūs* which God impressed on the celestial bodies have not been reduced or destroyed by the passage of time, because there was not, in celestial bodies, any inclination towards other movements, and there was no resistance which could corrupt and restrain these *impetūs*. All this I do not give as certain;

I would merely ask theologians to teach me how all these things could come about . . .'[13]

He went on to define the relation of his theory of *impetus* to other contemporary theories of motion. First he insisted that while the *impetus* of a projectile was an intrinsic principle of motion that inhered in the body it moved, it was a violent and unnatural principle impressed on the body by an extrinsic agent, and was opposed to the body's natural gravity. But what was *impetus?* It could not be identified with the motion itself, he argued evidently with an eye to Ockham, for the purpose of the theory was to propose a cause of the motion. So it was something distinct from the moving body. Nor could it be something purely transient, like motion itself, for this required a continuous agent to produce it. So, he concluded, 'this *impetus* is an enduring thing (*res naturæ permanentis*), distinct from local motion, by which the projectile is moved . . . And it is possible that this *impetus* is a quality designed by nature to move the body on which it is impressed, just as it is said that a quality impressed by a magnet on a piece of iron moves the iron to the magnet. And it is probable that just as this quality is impressed by the mover on the moving body together with the motion, so also it is decreased, corrupted and hindered, just as the motion is, by resistance [of the medium] or the contrary [natural] tendency.'

It has been claimed that by making *impetus* a *res permanens,* an enduring motive power that would maintain the body in motion unchanged so long as it was not acted on by forces that either diminished or increased it, Buridan took a strategic step towards the principle of inertia. Certainly his *impetus* was from this point of view an improvement on Marchia's *virtus,* which endured only *ad modicum tempus.* Certainly also there are striking resemblances between some of the basic definitions found in Buridan's

and in 17th-century dynamics. Buridan's measure of the *impetus* of a body as proportional to the quantity of matter and the velocity suggests Galileo's definition of *impeto* or *momento*, Descartes' *quantité de mouvement*, and even Newton's *momentum* as the product of mass multiplied by velocity. It is true that, in the absence of independent forces, Buridan's *impetus* would endure in a circle in celestial bodies, as well as in a straight line in terrestrial bodies, whereas Newton's momentum would persist only in a straight line in all bodies and required a force to bend it in a circle. But in this Galileo was not with Newton but stood somewhere between him and Buridan.

There is a certain resemblance also between Buridan's *impetus* and Leibniz's *'force vive,'* or kinetic energy. In explaining the acceleration of freely falling bodies, Buridan said in his *Quæstiones de Cælo et Mundo*, book 2, question 12: 'it must be imagined that a heavy body not merely acquires motion from its primary mover, namely from its gravity, but that it also acquires in itself a certain *impetus* together with that motion, which has the power of moving that same body, together with the constant natural gravity. And because this *impetus* is acquired commensurately with the motion, therefore the faster the motion, the greater and stronger is the *impetus*. So, therefore, the heavy body is initially moved only by its natural gravity, and hence slowly, but afterwards it is moved by the same natural gravity and simultaneously by the *impetus* that has been acquired, and thus it is moved more rapidly; . . . and so again it is moved more rapidly, and thus it is continuously accelerated always, to the end.' Some people, he concluded, call this *impetus* 'accidental gravity.'

It is interesting to look for analogies between terms appearing in systems of dynamics so widely separated in time, but these can also hide from us the gap that may separate their content. Can it really be said that Buridan's formulation of the theory of *impetus* implied the 17th-century definition of force as that which did not simply maintain velocity but altered it? Everything Buridan wrote about *impetus* indicates that he was proposing it as an Aristotelian cause of motion that should be commensurate with

the effect; therefore if the velocity increased, as in falling bodies, so must the *impetus*. It is true that, as a result of his attempt at quantitative formulations, Buridan's *impetus* can be seen as something more than an Aristotelian cause, as a force or power possessed by a body, by reason of being in motion, of altering the state of rest or motion of other bodies in its path. It is true also that there is too much similarity between this and the definition of *impeto* or *momento* given by Galileo in his *Two New Sciences* for it to be supposed that he owed nothing to Buridan (cf. below, p. 152 *et seq.*). But considering it in its own period, and not as a precursor of something in the future, it is clear that Buridan himself saw his theory as a solution of the classical problems that arose within the context of Aristotelian dynamics, from which he never escaped.

This is illustrated by the most suggestive question 9 of book 12 of his *Quæstiones in Libros Metaphysicæ*. 'Many people posit that the projectile, after leaving the projector, is moved by an *impetus* given by the projector, and that it is moved as long as the *impetus* remains stronger than the resistance. The *impetus* would last indefinitely (*in infinitum duraret impetus*) if it were not diminished by a resisting contrary, or by an inclination to a contrary motion; and in celestial motion there is no resisting contrary, so that when, at the creation of the world, God moved any sphere with whatever velocity he wished, he ceased from moving and that motion endured forever afterwards because of the *impetus* impressed on that sphere. Hence it is said that God rested on the seventh day from all the works that he had performed.' Did this mean that *impetus* would in fact always endure forever in all bodies in the absence of opposing forces? Buridan asserts this only for the celestial bodies, whose continuing motion was naturally circular. But in terrestrial bodies the *impetus* impressed violently, for example on a projectile, would always be opposed by the intrinsic natural tendency of the body towards its natural place, there to come to rest. Moreover, according to the basic dynamical law, which Buridan accepted in Bradwardine's formulation, that velocity was proportional to power and resistance, if there were no resist-

ance, velocity would be infinite. Sharing the empiricism common to all Aristotelians, Buridan did not consider abstracting the effects of *impetus* alone from those of its interaction with natural tendencies and with resistance. He stayed close to the actual world as he saw it. He did not conceive the principle of inertial motion in empty space.

But in a profound sense Buridan and his contemporaries did anticipate the great cosmological reform of the 16th and 17th centuries. Buridan's theory of *impetus* was an attempt to include both terrestrial and celestial movements in a single system of mechanics. In this he was followed by Albert of Saxony, Marsilius of Inghen and Nicole Oresme; although Oresme, holding that in the terrestrial region there were only accelerated and retarded motions, adapted the theory of *impetus* to this assumption and seems to have regarded it not as a *res naturæ permanentis* but as something that 'lasted only for a certain time.' In one form or another the theory became widely accepted in the 14th, 15th and 16th centuries in France, England, Germany and Italy.

As to questions of terrestrial dynamics, Buridan himself explained the bouncing of a tennis ball by analogy with the reflection of light, by saying that the initial *impetus* compressed the ball by violence when it struck the ground, and when it sprang back this imparted a new *impetus* which caused the ball to bounce up.[14] He gave a similar explanation for the vibration of plucked strings and the oscillation of a pendulum.

Albert of Saxony used Buridan's theory in his explanation of the trajectory of a projectile by compound *impetus*, an idea which itself went back to the 2nd-century B.C. Greek astronomer, Hipparchus, whose account was preserved in Simplicius' commentary on *De Cælo*. According to Aristotelian principles an elementary body could have only one simple motion at any time, for a substance could not have two contradictory attributes simultaneously. If it did, one would destroy the other. Albert of Saxony

[14] By contrast Descartes in *La Dioptique* explained the reflection and refraction of light by analogy with the mechanics of a tennis ball. Cf. below, pp. 118, 254.

held that the trajectory of a projectile was divided into three periods: (1) an initial period of purely violent motion during which the impressed *impetus* annihilated natural gravity; (2) an intermediate period of compound *impetus* during which movement was both violent and natural; and (3) a final period of purely natural movement vertically downwards after natural gravity and air resistance had overcome the impressed *impetus* (Fig. 1). He considered air resistance as having a definite frictional value even
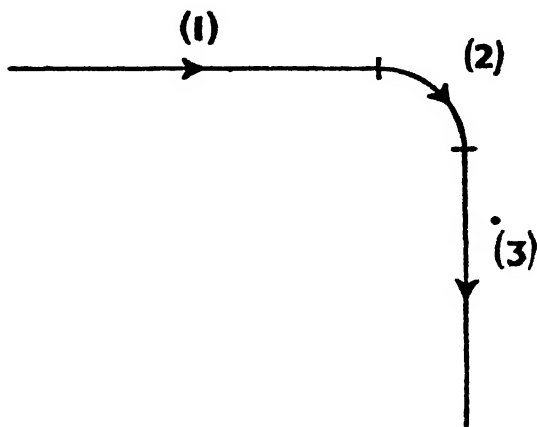

FIG. 1

when the projectile was at rest. In a horizontally fired projectile, motion during the first period was in a horizontal straight line until it suddenly curved during the second period to fall vertically in the third. When fired vertically upwards the projectile came to rest during the second period (or *quies media*) and then descended when natural gravity overcame air resistance. This theory was accepted by Blasius of Parma (d. 1416), Nicholas of Cusa, Leonardo da Vinci and other followers of Albert of Saxony, until it was modified in accordance with mathematical principles by Tartaglia in the 16th century and finally replaced by Galileo in the 17th.

The most significant developments of the new dynamics in the celestial region took place in application to the possibility of the daily rotation of the earth on its axis (cf. Vol. I, p. 90). This had been discussed and rejected in the 13th century by two Persian astronomers, al-Katibi and al-Shirazi, though no connection has been established between them and the Latin writers of the 14th century. For the latter the question involved not only the dynamical explanation of the persistence of motion, but also the conceptions of space and of gravitation. The most important writers to discuss the possibility of the motion of the earth and to relate it to these cognate problems were Buridan and Oresme. The frequency with which they referred to the Parisian condemnations of 1277 is a further illustration of the significance of these in the scientific speculations of the years that followed (cf. above, p. 36).

In his *Quæstiones de Cælo et Mundo* Buridan mentioned that many people held that the diurnal rotation of the earth was probable, though he added that they proposed this possibility as a scholastic exercise. He realised that immediate observation of the bodies themselves could not decide whether the heavens or the earth were in motion, but he rejected the motion of the earth on the grounds of the observations. For example, he pointed out that an arrow shot vertically fell to the place from which it was shot. If the earth revolved, he said, this would be impossible; and as for the suggestion that the revolving air would carry the arrow round, he replied that the *impetus* of the arrow would resist the lateral motion of the air.

The case made out by Oresme for the earth's diurnal rotation was far more elaborate. He discussed the problem in his *Livre du Ciel et du Monde,* a French commentary on Aristotle's *De Cælo* written in 1377 by command of Charles V of France, who commissioned him also to translate from the Latin into French Aristotle's *Ethics, Politics,* and *Economics.*[15] A lover of learning and of his own lan-

guage, Charles' *cabinet de livres* at the Louvre contained a large number of books translated into the vernacular at his own command, and these he encouraged the members of his entourage to read for their education and enjoyment. Although he concluded his *Livre du Ciel* by deciding in favour of the geostatic system, Oresme's analysis of the whole question was the most detailed and acute made between the Greek astronomers and Copernicus. In its treatment of the mixture of scientific, philosophical and theological issues involved it foreshadowed the controversial writings of Galileo.

An important question discussed by Oresme in expounding the geostatic system was that of the constant motion of the spheres. Since his version of the *impetus* theory could not account for constant motion, he fell back on a vague theory of a balance between 'motive qualities and powers' which God gave to the spheres at the creation to correspond to the gravity (*pesanteur*) of terrestrial bodies, and commensurate 'resistance' which opposed these powers (*vertus*). In fact he said that at the creation these powers and resistances had been bestowed by God upon the 'Intelligences' that moved the heavenly bodies; the Intelligences moved with the bodies whose movement they caused and were related to them as the human soul was to the body. Comparing the celestial machine to a clock, he concluded, in book 2, chapter 2 of *Le Livre du Ciel*:

> And these powers are so controlled, tempered and harmonised with the resistances that the movements are made without violence; and apart from violence, it is not in the least like a man making a clock and letting it go and be moved by itself. Thus God left the heavens to be moved continually in accordance with the proportions which their motive powers have to their resistances, and with the established order.

But was it possible to accept the assumptions on which the geostatic system, and the traditional objections to the earth's movement, were based? One of the essential assumptions of Aristotle's cosmology was that there must be at the centre of the universe a fixed body about which the

celestial spheres revolved and in relation to which the natural movements of terrestrial bodies took place. Against this Oresme argued that the directions of space, motion, and natural gravitation and levitation must, in so far as they were observable, all be regarded as relative.

Oresme agreed with those who argued that God, by his infinite power, could create an infinite space and as many universes as he chose. 'And so,' he wrote in book 1, chapter 24 of *Le Livre du Ciel*, 'beyond the sky is an empty, incorporeal space quite different from ordinary full and corporeal space, just exactly as the duration known as eternity is quite different from temporal duration, even if it were perpetual . . . Further, this space mentioned above is infinite and indivisible and is the immensity of God and is even God, just as the duration of God known as eternity is infinite and indivisible and even God . . .'

So far as directions were distinguished within our universe, Oresme showed that, considering right and left, before and behind. 'these 4 differences in the sky are not absolutely and really distinct, but only relatively, as it is said' (book 2, chapter 6). Only up and down could be said to be absolutely and really distinct, but then only relative to a particular universe. We could, for example, distinguish up and down according to the motion of light and heavy bodies. 'I say then that high and low in this . . . way are nothing else but the natural order of heavy and light things, which is such that all the heavy things, so far as is possible, are in the middle of the light things, without determining any other immovable place for them' (book 1, chapter 24). By combining this Pythagorean or Platonic theory of gravity with the conception of infinite space, Oresme was thus able to dispense with a fixed centre of the universe to which all natural gravitational movements were related. Gravity was simply the tendency of heavier bodies to go to the centre of spherical masses of matter. Movements were produced by gravity only relative to a particular universe; there was no absolute direction of gravity applying to all space.

There was then no ground for arguing that, supposing that the skies revolved, the earth must necessarily be fixed

in the centre. On the analogy of a revolving wheel, Oresme
showed that it was only necessary in circular motion that
an imaginary mathematical point in the centre be at rest,
as was in fact assumed in the theory of epicycles. Moreover
he said that it was not part of the definition of local mo-
tion that it should be referred to some fixed point or body.
For example, 'beyond the universe is an space conceived
as infinite and immobile, and it is possible without con-
tradiction for the whole universe to be moved in this space
in a straight line. And to say the contrary is an article
condemned in Paris. This postulated, there is no other
body to which the universe is related in any other way
according to place . . . Further, imagining that the earth
was moved through space for one day of daily motion and
that the heavens were at rest, and after this time that things
were again as they were' (book 2, chapter 8): then every-
thing would again be as it was before.'

In chapter 25 of book 2 of *Le Livre du Ciel* Oresme
said that, 'subject to correction,' it seemed to him possible
to maintain the opinion 'that the earth is moving with
daily motion and the heavens not. And first, I will declare
that it is impossible to show the contrary by any observa-
tion (*expérience*); secondly, from reason (*par raisons*); and
thirdly, I will give reasons in favour of the opinion.' The
objections which Oresme quoted against the earth's mo-
tion had all been mentioned by Ptolemy and were to be
used against Copernicus; he met them with arguments that
were to be used again by Copernicus and by Bruno.

The first objection from experience was that the skies
were actually observed to revolve about their polar axis.
To this Oresme replied, citing the fourth book of Witelo's
Perspective, that the only motion that could be observed
was relative motion. 'I assume that local motion cannot be
observed except in so far as a body can be seen to change
its position in respect to another body. Thus, if a man is
in a boat A, moving very smoothly, either fast or slowly,
and he can see nothing outside except another boat B,
moving in exactly the same way as the boat A in which he
is, I say that it will seem to this man that neither of the
boats is moving. If A is at rest and B is moving, it will

seem to him that B is moving; and if A is moving and B is at rest, it will seem to him just as before that B is moving. And so if A was at rest for an hour and B was moving, and then in the next hour, *e converso*, A was moving and B remained at rest, this man would not be able to perceive this change or variation, but it would seem to him all the time that B was moving; and this is evident from experience . . . It would seem to us all the time that the place where we are was at rest and that the other always moved, just as it seems to a man in a moving boat that the trees outside are moving. Similarly, if a man was on the sky, supposing that he was moving with daily motion, . . . it would seem to him that the earth was moving with daily motion, just as the sky seems to be, to us on the earth. Similarly, if the earth were moving with daily motion and the sky was not, it would seem to us that the earth was at rest and that the sky was moving. Any intelligent person can easily imagine this.'

The second objection from experience was if the earth were turning through the air from west to east, there should be a continuous strong wind blowing from the east. To this Oresme replied that the air and the water would share the earth's rotation, so that there would be no such wind. The third objection was that which had convinced Buridan: that if the earth were rotating an arrow or stone sent vertically upwards should be left behind to the west when it fell, whereas in fact it fell to the place whence it was sent up. Oresme's answer to this was profoundly significant. He said that the arrow 'is moved very rapidly eastwards with the air through which it goes and with the whole mass of the inferior part of the universe indicated before which is moved with daily motion, and thus the arrow returns to the place on the earth from which it was sent.' The arrow would in fact have not one movement but two, a vertical movement from the bow, and a circular movement from being on the rotating globe. The actual trajectory of the arrow, he said, would be comparable with that of a particle of fire (*a*) which rose from one position to a higher one nearer the celestial spheres. This he illustrated with a diagram, showing that the particle of fire

would not simply rise to a position *b* directly above *a*, but as it rose would be carried laterally by the circular motion to the position *c* to one side of *b*. 'I say that just as in the case of the arrow discussed above, so in this case it can be said that the motion of *a* is composed (*composé*) partly of a rectilinear motion and partly of a circular motion, for the region of the air and the sphere of fire through which *a* passes are moving, according to Aristotle, with circular motion. If they were not so moved, *a* would rise straight up on the line *ab*; but since *b* is meanwhile translated, by daily circular motion, to the point *c*, it is clear that as it rises *a* describes the line *ac*, and that the motion of *a* is composed of rectilinear and circular motion. The motion of the arrow will be of the same kind, as has been said; it will be a composition or mixture of motions (*composition ou mixcion de movemens*) . . .'[16] Thus, just as to a person on a moving ship any movement rectilinear with respect to the ship appears rectilinear, so to a person on the earth the arrow would appear to fall vertically to the point from which it was fired. The movement would appear the same to an observer on the earth whether the earth rotated or was at rest. 'I conclude then that it is impossible to show by any observation that the heavens are moving with daily motion and that the earth is not moving in this way.' This conception of the composition of movements was to become one of the most fruitful in Galileo's dynamics.

The objections 'from reason' against the earth's motion came mainly from the Aristotelian principle, used later by Tycho Brahe against Copernicus, that an elementary body could have only one simple movement which, for earth, was rectilinearly downwards. Oresme asserted that each of the elements except the skies might well have two natural movements, one being rotation in a circle when it was in its natural place, and the other being rectilinear motion by which it returned to its natural place when displaced from it. The '*vertu*' that moved the earth in rotation was its 'nature' or 'form,' just as was that which moved it recti-

---

[16] This would seem incompatible with acceptance of the three-fold division of the trajectory of a projectile: see above, p. 74.

linearly back to its natural place. As for the objection that the earth's rotation would ruin astrology, Oresme replied that all the calculations and tables would be just as before.

The main positive arguments that Oresme brought in favour of the earth's rotation all turned on this being simpler and more perfect than the alternative, once more a striking anticipation of the arguments, Platonic in inspiration, that were to be used by Copernicus and Galileo. If the earth rotated, he said, all the apparent celestial motions would take place in the same sense, from east to west; the habitable part of the globe would be on its right or nobler side; the heavens would enjoy the nobler state of rest and the base earth would move; the more distant celestial bodies would make their revolutions proportionately more slowly than those nearer the earth, instead of more rapidly as in the geocentric system. Moreover, 'all philosophers say that anything done by many or by large operations that could be done by less or smaller operations would be done in vain. And Aristotle says . . . that God and Nature do nothing in vain. . . . And so, since all the effects which we see can be produced and all appearances saved by one small operation, namely the daily motion of the earth, which is very small compared with the heavens, without so multiplying operations which are so diverse and outrageously large, it follows that God and Nature would have made and ordered such operations for nothing, and that is not fitting, as the saying goes.' Among the advantages of simplicity was that the ninth sphere would no longer be necessary.

Throughout his discussions Oresme, the Bishop, after all, of Lisieux, had taken into account the support apparently given to the geostatic system by many passages of Scripture, but these he had turned by remarking, for example: 'One can say that it (scil. Scripture) conforms in this part to the manner of common human speech, just as it does in several places, as where it is written that God repented and that he became angry and calm again, and things of the same kind, which are not in fact at all as the letter puts it.' Again we are reminded of Galileo, and in the same spirit Oresme dealt with the celebrated problem

of Joshua's miracle and asserted that no arguments could be found against the earth's motion.

'When God performs any miracle, it must be supposed and held that he does this without disturbing the common course of nature more than the least that is necessary for the miracle. And so, if one can say that God lengthened the day in the time of Joshua by stopping only the motion of the earth or the inferior region, which is so small, indeed a mere point compared with the heavens, without bringing it about that the whole universe outside this little point has been put out of its common course and order, and likewise the heavenly bodies, then this is much more reasonable . . . and one can say the same thing about the sun going back on its course in the time of Ezekiel.'

After finally reviewing all the arguments he has brought against the accepted cosmology, it is somewhat surprising to find Oresme concluding his chapter by returning to it once more. 'Nevertheless everyone holds and I think that it (*scil.* the heavens) is moving and not the earth: For God fixed the earth, so that it does not move (*Deus enim firmavit orbem terre, qui non commovebitur*[17]), notwithstanding the reasons to the contrary, for these are persuasive arguments that do not prove evidently. But considering everything that has been said, one could believe from this that the earth is moving and not the heavens, and there is nothing evident to the contrary. In any case this seems *prima facie* as much contrary to natural reason as the articles of our faith, or more so, all or several. And so what I have said for amusement (*par esbatement*) can in this way acquire a value for confuting and regaining those who want to use reason to call our faith in question.' Was this last remark related to the purpose for which Oresme in his concluding chapter said he composed *Le Livre du Ciel*: 'to stimulate, excite and move the hearts of young men of fine and noble intelligence and with a desire for knowledge, so that they will study to contradict and correct me, for love and affection for the truth'? On the issue, so delicate, so fundamental and so passionate in Western thought from

[17] *Vulgate*, Psalm 92. 'The world also is stablished, that it cannot be moved'. (Authorised Version, Psalm 93.)

the arrival of the new Aristotle in the 13th century down to the controversies of Galileo, of the relation of reason to revelation, of the cosmology of natural science to the cosmology of Scripture, Oresme seems to have taken a position not uncommon among contemporaries who were at once Christian believers and philosophical sceptics. He was prepared to submit reason unconditionally to revelation, and at the same time to use reason to confound reason. 'And all this I say and put forward without insistence, from great humility and fearfulness of heart, saluting always the majesty of the Catholic faith, and in order to hold in check the curiosity or presumption of any of those who, perhaps, might want to slander or attack it or to inquire too boldly, to their confusion.'

But whatever the reasons why Oresme finally rejected the cosmology of the earth's motion in support of which he gave so many arguments, he leaves no doubt about his final opinion. 'But in fact there never was and never will be but a single corporeal universe,' he declared in chapter 24 of book 1 of *Le Livre du Ciel*; that Universe was the accepted geostatic one of Aristotle and Ptolemy. And indeed, as Oresme well understood, none of his arguments positively proved the motion of the earth; he declared simply, as Galileo was to declare three centuries later, that he had shown that it was impossible to prove the contrary. But Oresme's conception of motion did not contain the dynamical potentialities that Galileo was to exploit, however unsuccessfully, in the cosmological debate. His conception of relative motion in fact resembled that of Descartes in ignoring what came to be called the inertial properties of matter. It provided him with no criteria for deciding between dynamically possible and impossible astronomical systems.

Albert of Saxony claimed in his *Quæstiones in Libros de Cælo et Mundo*, book 2, question 26:

> we cannot in any manner, by the movement of the earth and the repose of the sky, save the conjunctions and oppositions of the planets, any more than the eclipses of the sun and moon.

But in fact, as Oresme said in book 2, chapter 25 of his commentary, in pointing out that astrology would not be affected by the earth's rotation, 'all conjunctions, oppositions, constellations, figures and influences of the sky would be just as they are, in every way, . . . and the tables of movements and all other books would be as true as they are now, except only that one would say that the daily movement is apparent in the heavens and real in the earth.' It was for philosophical and physical reasons that astronomers continued to use the geostatic hypothesis, and natural philosophers did no more than toy with alternatives. For example, Nicholas of Cusa (1401–64) in the next century threw out the suggestion that in every twenty-four hours the eighth sphere revolved twice about its poles while the earth revolved once. Oresme's treatise was never printed and it is not known whether Copernicus ever saw it. The question of plural worlds on which, for instance, Leonardo da Vinci sided with Nicholas of Cusa against Albert of Saxony, continued to excite passionate debates at the end of the 15th century and long afterwards, and these authors were read in northern Italy when Copernicus was at Bologna and Padua. Cusa had given Buridan's dynamics a Platonic twist by attributing the permanence of celestial rotation to the perfect spherical form of the spheres. The circular movement of a sphere on its centre would continue indefinitely, he said in his *De Ludo Globi*, and just as the movement given to the ball in a game of billiards would continue indefinitely if the ball were a perfect sphere, so God had only to give the celestial sphere its original *impetus* and it has continued to rotate ever since and kept the other spheres in motion. This explanation was adapted by Copernicus for his system. By giving the earth and planets an annual motion round the sun Copernicus offered a mathematical as well as physical alternative to Ptolemy. When he came to consider gravitation and the other physical problems involved, his work appears as a direct development of that of his predecessors.

(4) MATHEMATICAL PHYSICS
IN THE LATER MIDDLE AGES

One of the most important changes facilitating the increasing use of mathematics in physics was that introduced by the theory that all real differences could be reduced to differences in the category of quantity; that, for example, the intensity of a quality, such as heat, could be measured in exactly the same way as could the magnitude of a quantity. This change was what chiefly distinguished the mathematical physics of the 17th century from the qualitative physics of Aristotle. It was begun by the scholastics of the later Middle Ages.

As with so many scientific concepts in the Middle Ages, the problem was first discussed in a theological context and the principles worked out there were later applied to physics. It was Peter Lombard who opened the question by asserting that the theological virtue of charity could increase and decrease in a man and be more or less intense at different times. How was this to be understood? Two schools of thought developed, one supporting Aristotle's view of the relations of quality to quantity and the other opposing it.

For Aristotle, quantity and quality belonged to absolutely different categories. A change in quantity, for instance growth, was brought about by the addition of either continuous (length) or discontinuous (number) homogeneous parts. The larger contained the smaller actually and really and there was no change of species. Although a quality, for instance heat, might exist in different degrees of intensity, a change of quality was not brought about by the addition or subtraction of parts. If one hot body was added to another the whole did not become hotter. A change of intensity in a quality therefore involved the loss of one attribute, that is, one species of heat, and the acquisition of another. This was the view, for example, of Aquinas.

Those who, in the 14th century, took the opposite side

to Aristotle in this discussion of the relation of quality to quantity, or, as it was called, the 'intension and remission of qualities or forms' (*intensio et remissio qualitatum seu formarum*), maintained that when two hot bodies were brought into contact, not only the heats but also the bodies were added together. If it were possible to abstract the heat from one body and add it alone to another body, the latter would become hotter. In the same way if it were possible to abstract the gravity from one body and add it to the mass of another body, the latter would become heavier. It was thus asserted, and supported by the authority of Scotus and Ockham, that the intensity of a quality such as heat was susceptible to measurement in numerical degrees, in the same way as the magnitude of a quantity.

Aristotle had analysed physical phenomena into irreducibly, qualitatively, different species, but mathematical physics reduces the qualitative differences of species to differences of geometrical structure, number and movement, in other words, to differences of quantity, and for mathematics one quantity is the same as another. 'I hold that there exists nothing in external bodies for exciting in us tastes, odours and sounds except sizes, shapes, numbers and slow or swift motions,' Galileo was to declare famously in *Il Saggiatore* (question 48); (cf. below, pp. 300–2), matching the equally famous exclamation of Descartes: '*Qu'on me donne l'étendue et le mouvement, et je vais refaire le monde. . . . l'univers entier est un machine où tout se fait par figure et mouvement.*' The origin of this idea is to be found in Pythagoras and in Plato's *Timæus*, well known throughout the Middle Ages, and it was the Platonists who were mainly responsible for developing it in the Middle Ages, as later in the 17th century.

Grosseteste, for example, in developing his theory of the 'multiplication of species' (cf. Vol. I, pp. 74, 99–100, above, p. 21), distinguished between the physical activity by which the *species* or *virtus* were propagated through the medium and the sensations of light or heat which they produced when they acted on the appropriate sense organs of a sentient being. The physical activity was independent, as he put it in *De Lineis*, of 'whatever it may meet, whether

something with sense perception or something without it, whether something animate or inanimate; but the effect varies with the recipient.'[18] For, he went on, 'when received by the senses this power produces an operation in some way more spiritual and more noble; on the other hand when received by matter, it produces a material operation, as the sun by the same power produces diverse effects in different subjects, for it cakes mud and melts ice.' In this passage Grosseteste was in effect implying a distinction between primary and secondary qualities in the same sophisticated manner as this was made in the 17th century; the distinction became methodologically and metaphysically significant in physics when the primary qualities were attributed to a physical activity that need not be directly observable (cf. below, pp. 142, 303 *et seq.*).

The physical mode of operation of the fundamental material substance and power, which he held to be light, he conceived to be by means of a succession of pulses or waves on the analogy of sound, and he attempted to express this activity and its diversified effects in mathematical form (cf. Vol. I, p. 103). A similar distinction between light as sensation and light as an external physical activity to be expressed geometrically was made by Roger Bacon, Witelo, and Theodoric of Freiberg. Though no medieval writer seems to have conceived the fundamental idea that different colours as perceived were correlated with anything corresponding to the 'wave-length' of light, the optical writers did propose that the differences in the qualitative effects of light were produced by quantitative differences in the light itself. For example, Witelo and Theodoric of Freiberg said that the colours of the spectrum—each a different species of colour according to a strict Aristotelian view —were produced by the progressive weakening of white light by refraction (cf. Vol. I, pp. 110–11). Grosseteste correlated the intensity of illumination and of heat with the angle at which the rays were received and their concentra-

tion. John of Dumbleton was to attempt to formulate a quantitative law relating intensity of illumination to distance.

As Roger Bacon expressed the point in his *Opus Majus* (part 4, distinction 1, chapter 2), 'all categories depend on a knowledge of quantity, concerning which mathematics treats, and therefore the whole excellence of logic depends on mathematics.' In medical writings also it became a commonplace to discuss Galen's suggestion that heat and cold should be represented in numerical degrees. There was a general move in many different fields towards finding means of representing qualitative differences by concepts that could be expressed quantitatively and manipulated by mathematics. The interest of the scholastics was seldom directed purely towards solving actual scientific problems. They were nearly always primarily interested in some question of principle in natural philosophy or method, and if particular scientific problems were tackled, it was nearly always so to speak accidentally by way of illustration of a more general quasi-philosophical point. But it is nevertheless possible to see in the 14th-century discussions the origins of some of the most powerful procedures of mathematical physics that became fully effective only in the 17th century. At the same time motion, where the statically conceived Greek geometry had been impotent, was first treated mathematically, thus leading to the foundation of the science of kinematics, that is, the analysis of movement in terms of distance and time.

The new methods of mathematical physics were developed in the first place in connection with the idea of functional relationships. This is the natural complement of a systematic conception of concomitant variations between cause and effect; by expressing the phenomenon to be explained (the dependent variable as we now call it) as an algebraic function of the conditions necessary and sufficient to produce it (the independent variables), it can be shown precisely how changes in the former are related to changes in the latter. To be effective in practice the method depends on making systematic measurements, and these were few and far between before the 17th century, although

some were made, for example in astronomy, and in Wite-lo's account of the systematic variation of angles of refraction with angles of incidence of light (see Vol. I, p. 110). In the 14th century the idea of functional relationships was developed without actual measurements and only in principle; that represented the extent of contemporary interest in this as in most other aspects of scientific method.

Two main methods of expressing functional relationships were developed. The first was the 'word-algebra' used in mechanics by Bradwardine at Oxford, in which generality was achieved by the use of letters of the alphabet instead of numbers for the variable quantities, while the operations of addition, division, multiplication, etc. performed on these quantities were described in words instead of being represented by symbols as in modern algebra (cf. above, p. 56 *et seq.*; below, pp. 128–29). Bradwardine was followed in this method at Oxford by numerous writers of treatises on 'proportions,' and by a group at Merton College during the 1330s and 1340s known as the *calculatores*, especially William of Heytesbury (*c.* 1313–72), Richard Swineshead (*fl.c.* 1344–54), the author of the *Liber Calculationum* who was specifically known as *Calculator*,[19] and John of Dumbleton (*fl.c.* 1331–49). None of these Oxford writers seems to have been interested in the dynamical aspects of motion; indeed, apparently under the influence of Ockham and Bradwardine, Swineshead and Dumbleton specifically rejected the theory of *virtus impressa*, without adopting Buridan's alternative

I am indebted to Dr. J. A. Weisheipl for the following note distinguishing this Richard Swineshead from two contemporaries, John and Roger, who also bear the place-name of Swineshead. It would seem that John, also a Fellow of Merton College (*c.* 1343–5?), became a lawyer, but no writings of his are known. Roger wrote the treatise *De Motibus Naturalibus*, 'datus Oxonie ad utilitatem studencium' (Erfurt MS Amplon. F.135, f.47), and probably the well-known logical text-book *De Insolubilibus et Obligationibus* before 1340; nothing is known about him, but he may have become a Benedictine monk of Glastonbury and Master in Sacred Theology, the *subtilis Swynyshed, proles Glastoniæ*, of Richard Tryvytlam's poem in *Collectanea* (vol. 3, ed. M. Burrows). The date of his death is given as 1365 in British Museum MS Arundel 12, f.80.

theory of *impetus*. It was in Paris that Bradwardine's meth-
ods were developed in the context of a physical dynamical
theory, and all the principal writers on *impetus* show his
direct influence and used his dynamical function: Buridan
himself, Oresme, Albert of Saxony, Marsilius of Inghen.

Applied to the problem of giving quantitative expression
to changes of quality, the problem of *intensio et remissio
qualitatum seu formarum* or 'the latitude of forms' (*lati-
tudo formarum*) as it was called, the purpose of the meth-
ods developed at Oxford was to express the amounts by
which a quality or 'form' increased or decreased numeri-
cally in relation to some fixed scale. A 'form' was any varia-
ble quantity or quality in nature, for example local mo-
tion, growth and decrease, qualities of all kinds, or light
and heat. The intensity (*intensio*) or 'latitude' of a form
was the numerical value that was to be assigned to it, and
thus it was possible to speak of the rate at which the *in-
tensio*, for example of velocity or of heat, changed in re-
lation to another invariable form known as the 'extension'
(*extensio*) or 'longitude' (*longitudo*), for example dis-
tance or time or quantity of matter. A change was said to
be 'uniform' when, as in uniform local motion, equal dis-
tances were covered in equal successive intervals of time,
and 'difform' when, as in accelerated or retarded motion,
unequal distances were covered in equal intervals of time.
Such a 'difform' change was said to be 'uniformly difform'
when the acceleration or retardation was uniform; other-
wise it was 'difformly difform.'

It was this conception of the relationship between the
intensio and *extensio* of forms that gave rise to the second
method of repressing functional relationships in the 14th
century, a geometrical method by means of graphs. The
Greeks and Arabs had sometimes used algebra in connec-
tion with geometry, and the idea of plotting the position
of a point in relation to rectangular co-ordinates had been
familiar to geographers and astronomers since classical
times (cf. Pl. II). The graphical representation of the
degrees of *intensio* of a quality against *extensio* by means
of rectilinear co-ordinates had become fairly common in

both Oxford and Paris by the early years of the 14th century. Representing *extensio* by a horizontal straight line (*longitude*), each degree of *intensio* corresponding to a given *extensio* was represented by a perpendicular vertical line (*latitudo vel altitudo*) of specified height. The line connecting the summits of these 'latitudes' could then assume different shapes. For example, if velocity ('intensity or latitude of motion') were plotted against time ('longitude'), uniform velocity would be represented by a horizontal straight line at a height corresponding to the velocity; uniformly difform velocity (i.e. uniform acceleration or retardation) by a straight line making an angle with the horizontal; difformly difform velocity (i.e. changing acceleration or retardation) by a curve.

One of the first to use this geometrical method was Dumbleton, who discussed the subject in his *Summa Logicæ et Philosophiæ Naturalis*, a vast critical discussion of most of the major topics of contemporary physics. In the second part of this important work[20] Dumbleton made an interesting distinction between a change in quality 'in reality and in name,' asserting that in fact no species of quality really changed, but that each degree of intensity was a different species; the mathematical methods gave merely a quantitative and 'nominal' representation of such differences. In the fifth part of the *Summa* he applied the method to the problem of the variation of the intensity or strength of action of light with distance from the source. There can be few writers in any period whose argument is more difficult to follow than Dumbleton's, but in the course of a succession of propositions, objections, objections to objections, going on almost endlessly, he did begin the analysis of some basic questions of optics that were only answered in the 17th century. He said that the intensity of illumination at a given point was directly proportional to the strength of the luminous source and inversely proportional to the 'density' of the medium. With a given source and medium he said that the intensity of illumination decreased with distance but

[20] Cambridge MS Peterhouse 272; Oxford MS Merton 306; both 14th cent.

not 'uniformly difformly,' that is, not in simple proportion. It was Kepler who in his *Ad Vitellionem Paralipomena* (1604) first formulated the photometric law according to which the intensity of illumination is proportional to the inverse square of the distance from the source (see below, p. 194).

The graphical method of representing the 'latitude of forms' was used in Paris in connection with kinematic problems by Albert of Saxony and Marsilius of Inghen, but the most striking advances were made by Oresme. There are many examples of Oresme's originality as a mathematician: he conceived the notion of fractional powers, afterwards developed by Stevin (cf. below, p. 129), and gave rules for operating them. It has been claimed that he anticipated Descartes in the invention of analytical geometry. Leaving aside the obscure question whether Descartes had any actual direct or indirect knowledge of Oresme's work, it is clear from the latter itself that Oresme had other ends in view than those of the 17th-century mathematician.

Following the common practice, Oresme represented *extensio* by a horizontal straight line and made the height of perpendiculars proportional to *intensio*. His object was to represent the 'quantity of a quality' by means of a geometrical figure of an equivalent shape and area. He held that properties of the representing figure could represent properties intrinsic to the quality itself, though only when these remained invariable characteristics of the figure during all geometrical transformations. He even suggested the extension of these methods to figures in three dimensions. Thus Oresme's horizontal *longitudo* was not strictly equivalent to the abscissa of Cartesian analytical geometry: he was not interested in plotting the positions of points in relation to the rectilinear co-ordinates, but in the figure itself. There is in his work no systematic association of an algebraic relationship with a graphical representation, in which an equation in two variables is shown to determine a specific curve formed by simultaneous variable values of *longitudo* and *latitudo*, and *vice versa*. Nevertheless, his work was a step towards the invention of analytical geome-

try and towards the introduction into geometry of the idea of motion which Greek geometry had lacked. He used his method to represent linear change in velocity correctly.

According to the definitions given above, the velocity of a body moving with uniform acceleration would be uniformly difform with respect to time. Taking acceleration as 'the velocity of a velocity,' Heytesbury in his *Regulæ Solvendi Sophismata* defined uniform acceleration and uniform retardation very clearly as a movement in which equal increments of velocity were acquired, or lost, in any equal periods of time. He also gave an analysis and definition of instantaneous velocity, the measure of which he made (as Galileo was to later) the space that *would* be described by a point if it were allowed to move for some given time at the velocity it had at the given instant. Using these and similar definitions Heytesbury and his contemporaries at Merton College gave kinematic descriptions of various forms of movement, but one was to prove of special significance. Some time before 1335 (the date of Heytesbury's *Regulæ*) it was discovered at Oxford that a uniformly accelerated or retarded movement is equivalent, so far as the space traversed in a given time is concerned, to a uniform movement of which the velocity is equal throughout to the instantaneous velocity possessed by the uniformly accelerated or retarded movement at the middle instant of time. This was proved arithmetically by Heytesbury,[21] by Richard Swineshead, and by Dumbleton, and it may be called the Mean Speed Rule of Merton College. Oresme, in his *De Configurationibus Intensionum*, or *De Configuratione Qualitatum*, part 3, chapter 7, afterwards gave the following geometrical proof of this rule. He said:

> Any uniformly difform quality has the same quantity as if it uniformly informed the same subject according to the degree of the mid-point. By 'according to the degree of the mid-point' I understand if the quality be

[21] The proof is given in *De Probationibus Conclusionum* (Venice, 1494) attributed to Heytesbury, but the authenticity of this work is not beyond dispute. Swineshead's proof occurs in the *Liber Calculationum* and Dumbleton's in the *Summa*, both of which were written certainly after Heytesbury's *Regulæ*.

linear. For a quality of a surface it would be necessary to say: 'according to the degree of the middle line . . .'

We will demonstrate this proposition for a linear quality.

Let there be a quality which can be represented by a triangle, ABC (Fig. 2). It is a uniformly difform quality which, at point B, terminates at zero. Let D be the mid-point of the line representing the subject; the degree of intensity that affects this point is represented by the line DE. The quality that will have everywhere the degree thus designated can then be represented by the quad-rangle AFGB . . . But by the 26th proposition of Euclid,
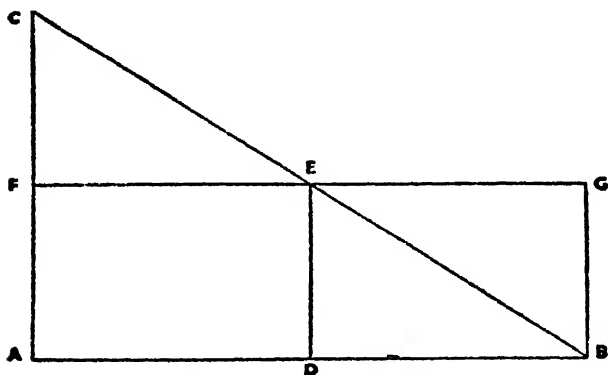
FIG. 2

Book I, the two triangles EFC and EGB are equal. The triangle, which represents the uniformly difform quality, and the quadrangle AFGB, which represents the uniform quality, according to the degree of the mid-point, are then equal. The two qualities which can be represented, the one by the triangle and the other by the quadrangle, are then also equal to one another, and it is that which was proposed for demonstration.

The reasoning is exactly the same for a uniformly dif-form quality which terminates in a certain degree . . .

On the subject of velocity, one can say exactly the same thing as for a linear quality, only, instead of saying: 'mid-point,' it would be necessary to say: 'middle instant of the time of duration of the velocity.'

It is then evident that any uniformly difform quality or velocity whatever is equalled by a uniform quality or velocity.[22]

The treatment of kinematic problems in the 14th century remained almost entirely in the realm of the theoretical. Especially in Oxford, problems were posed *secundum imaginationem*, as imaginary possibilities for theoretical analysis and without empirical application. In Paris the physical and dynamical context of the discussion did direct interest to the kinematics of actual natural motion, but this was treated largely without reference to observation or experiment. A good example is the treatment of the kinematics of freely falling bodies given by Albert of Saxony in his *Quæstiones in Libros de Cælo* (book 2, question 14). After discussing various possible ways in which the natural velocity of a freely falling body might increase with time and with the space traversed, he concluded that the velocity of fall increased in direct proportion to the distance of the fall.[23] This erroneous opinion was also to seduce Galileo before he decided on the correct solution, namely that velocity increased in direct proportion to the time of the fall, or in other words that a freely falling body moved according to Heytesbury's definition of uniformly accelerated velocity (see below, pp. 145–48). This correct solution was indeed *implied* elsewhere by Albert of Saxony, when like Buridan he said that the longer a movement took the more *impetus* was required and thus the more velocity was acquired. But he did not say this when discussing the kinematic problem and there is no evidence that he was himself aware of the kinematic implica-

tions of his dynamics. The correct law of acceleration in free fall was given, with considerable confusion, by Leonardo da Vinci and later unequivocally by the Spanish scholastic Domingo de Soto, and finally with quantitative deductions by Galileo.

Certainly the first two of these writers based their work either directly or indirectly on that of their 14th-century predecessors in Oxford and Paris, and Galileo also had a knowledge of the 14th-century kinematics and dynamics. The *calculatores* of Merton College in fact enjoyed a long period of considerable popularity, first at Paris and in Germany, then in Italy and especially at Padua in the 15th and 16th centuries, and again at Paris in the 16th. Between about 1480 and 1520 the new printing presses, especially of Venice and Paris, published editions of the most important writings of Heytesbury, Richard Swineshead and Bradwardine, and of Buridan and Albert of Saxony. Oresme's own principal writings escaped publication, but indirect knowledge of his kinematical theorems was available. Galileo in his *Juvenilia*, apparently notes on lectures by his master Francesco Bonamico at Pisa, mentioned, among many other medieval writers on physics, Burley, Heytesbury, Calculator, Albert of Saxony and Marliani. This does not, of course, imply that he read their books. He also mentioned Ockham and Soto, and Philoponus and Avempace; but the names of Buridan and Oresme do not occur.

Resolving Albert of Saxony's hesitations, Soto in 1545 took the velocity of free fall as proportional to time, and declared it to be 'uniformly difform,' that is, uniformly accelerated. The violent movement of a projectile fired vertically upwards he also declared to be 'uniformly difform,' but in this case uniformly retarded. To both he applied the Mean Speed Rule relating distance and time, thus transcending the qualitative difference between natural and violent motion by means of mathematics.[24] When Galileo finally stated the correct law of free fall and clearly

[24] Another fundamental aspect of falling bodies, that the acceleration is the same for all bodies of any substance, was first fully appreciated, though only slowly, by Galileo.

elucidated 'the intimate relationship between time and motion,' as he said in the Third Day of his *Two New Sciences* (1638), he used Oresme's theorem in setting out his proof (see below, p. 151).

But there is a world of difference between Galileo's discussion of free fall and that of his scholastic predecessors, and the main direction of the interests of the latter could not be better illustrated than by the contrast. Where the 14th-century scholastics had discussed possible kinds of motion with only the most casual references to empirical actuality, Galileo turned his attention firmly towards the motions actually found in nature as the real object whose elucidation was the main if not the only purpose of theoretical kinematic analysis. Between the 14th century and the 17th, scientific thinkers had transferred their main attention from questions of principle and possibility to questions of actual fact. 'For anyone may invent an arbitrary type of motion and discuss its properties,' Galileo wrote in a famous passage in the Third Day of the *Two New Sciences*; and the properties which these motions and curves possessed in virtue of their definitions might be interesting, even though not met with in nature. 'But we have decided to consider the phenomena of bodies falling with an acceleration such as actually occurs in nature and to make this definition of accelerated motion exhibit the essential features of observed accelerated motions.' And this, he concluded, he had eventually succeeded in doing, and was confirmed in this belief by the exact agreement of his theoretical definition with the results of experiments with a ball rolling down an inclined plane (see below, p. 145 *et seq.*).

The 14th-century attempt to express the quantitative equivalent of qualitative differences led to genuine discoveries concerning both mathematics and physical fact. The latter were extended by the encouragement given to physical measurement, although here ideas were ahead of practical possibilities determined by the scope and accuracy of the available instruments. For example, Ockham said that time could be considered objectively only in the sense that by enumerating the successive positions of a body moving

with uniform motion, this motion could be used to measure the duration of the motion or rest of other things. The sun's motion could be used to measure terrestrial movements, but the ultimate reference of all movement was the sphere of the fixed stars, which was the fastest and most nearly uniform motion there was. Other writers elaborated systems for measuring time in fractions (*minutæ*) and the division of the hour into minutes and seconds was in use early in the 14th century. Although mechanical clocks had come in during the 13th century, they were too inaccurate for measuring small intervals of time, and the water-clock and sand-glass continued to be used. The accurate measurement of long intervals was not possible before the invention of the pendulum clock by Huygens in 1657.

The idea of representing heat and cold in numerical degrees was also familiar to physicians. As a zero point Galen had suggested a 'neutral heat' which was neither hot nor cold. Since the only means of determining the degree of heat was by direct sense-perception and a person of hotter temperature would perceive this 'neutral temperature' as cold, and *vice versa*, he had suggested, as a standard neutral degree of heat, a mixture of equal quantities of what he regarded as the hottest (boiling water) and coldest (ice) possible substances. From these ideas Arab and Latin physicians developed the idea of scales of degrees, a 'popular scale being one ranging from 0 to 4 degrees of heat or of cold. Drugs also were supposed to have something analogous to a heating or cooling effect and were given their place on a scale. Natural philosophers adopted a scale of 8 degrees for each of the four primary qualities. Though in these attempts to estimate degrees of heat, it was known that heat caused expansion, the only thermometer was still the senses. Moreover, a fundamental conceptual difficulty can be seen in the attempt to measure both heat and cold. It was only when the classical conception of pairs of opposites—hot, cold; up, down; and all the rest—had been replaced by the concept of homogeneous linear measures that a workable system of measurement became possible for physics as a whole. The change was made first in mechan-

ics, and modern thermometry followed that example (cf. below, p. 153, note).

Besides the water-clock and sand-glass, the mechanical clock, the astronomical instruments already described, and such 'mathematical instruments' as the straight-edge, square, compass and dividers, the only other scientific measuring instruments available in the 14th and 15th centuries were, in fact, the rules, measures, balances and weights for employing the standards of length, capacity and weight recognised in trade. Balances of both the equal-arm and steelyard type date from antiquity and were used by alchemists and by assayers in metallurgy.

Further attempts to make use of measurement and experiment in science were made during the 15th century, when the scientific leadership of Europe passed from the Anglo-French universities to Germany and Italy. Attempts had been made in the 14th century to express the relationship between the elements graphically on a chart and to state the proportions of the elements and the degrees of the primary qualities for each of the metals, spirits (quicksilver, sulphur, arsenic, sal ammoniac), etc. In the fourth book of his *Idiota*, entitled *De Staticis Experimentis*, Nicholas of Cusa suggested that such problems should be solved by weighing. His conclusions imply the idea of the conservation of matter.

> *Idiot* . . . For weighing a piece of Wood, and then burning it thoroughly, and then weighing the ashes, it is knowne how much water there was in the wood, for there is nothing that hath a heavie weight but water and earth. It is knowne moreover by the divers weight of wood in aire, water and oyle, how much the water that is in the wood, is heavier or lighter than clean spring water, and so how much aire there is in it. So by the diversity of the weight of ashes, how much fire there is in them: and of the Elements may bee gotten by a nearer conjecture, though precision be always inattingible. And as I have said, of Wood, so may be done with Herbs, flesh and other things.

Orator. There is a saying, that no pure element is to be given, how is this to be prov'd by the Ballance?

Idiot. If a man should put a hundred weight of earth into a great earthen pot, and then should take some Herbs, and Seeds, and weigh them, and then plant or sow them in that pot, and then should let them grow there so long, untill hee had successively by little and little, gotten an hundred weight of them, he would finde the earth but very little diminished, when he came to weigh it againe: by which he might gather that all the aforesaid herbs, had their weight from the water. Therefore the waters being ingrossed (or impregnated) in the earth, attracted a terrestreity, and by the opperation of the Sunne upon the Herb were condensed (or were condensed into an Herb). If those Herbs bee then burn't to ashes, mayst thou not guesse by the diversity of the weights of all, how much earth thou foundest more than the hundred weight, and then conclude that the water brought all that? For the Elements are convertible one into another by parts, as wee finde by a glass put into the snow, where wee shall see the aire condensed into water, and flowing in the glass.[25]

The *Statick Experiments* contained several other suggested applications of the balance. One of these, the comparison of the weights of herbs with those of blood or urine, was directed towards understanding the action of medicines. This was investigated in a different way in the *Liber Distillandi* published by Hieronymus Brunschwig in Strassburg in 1500, in which it was recognised that the action of drugs depended on pure principles, 'spirits' or 'quintessences' which could be extracted by steam distillation and other chemical methods. Cusa also suggested that the time a given weight of water took to run through a given hole might be used as the standard of comparison for pulse rates. The purity of samples of gold and other metals, he said, could be discovered by determining their specific weights, using Archimedes' principle The balance might be used also to measure the 'virtue' of a lodestone

[25] Cusanus, *The Idiot in Four Books*, London, 1650.

attracting a piece of iron and, in the form of a hygrometer consisting of a piece of wool balancing a weight, to determine the 'weight' of the air. The same device was described by Leon Battista Alberti (1404–72) and by Leonardo da Vinci (1452–1519). The air might also be 'weighed,' Cusa said, by determining the effect of air resistance on falling weights while time was measured by the weight of water running through a small hole.

> Whether might not a man, by letting a stone fall from a high tower, and letting water run out of a narrow hole, into a Bason in the meane time; and then weighing the water that is runne out, and doing the same with a piece of wood of equall bignesse, by the diversity of the weights of the water, wood, and stone, attain to know the weight of the aire?

Cusa's suggestions were sometimes a little vague and it is rather tantalising that the last experiment should have been described without reference to the dynamics of falling bodies. This problem was taken up, suggestively but inadequately, by the Italian doctor Giovanni Marliani (d. 1483). Marliani had made some observations on heat regulation in discussing the intensity of heat in the human body. He developed Bradwardine's modification of Aristotle's law of motion. In criticising the Aristotelian law he mentioned experiments based on dynamical deductions from the statics of Jordanus Nemorarius, which had been kept alive at Oxford, and had been made known to the Italians by the *Tractatus de Ponderibus* of Blasius of Parma (d. 1416). Marliani noted in his *De Proportione Motuum in Velocitate* that the period of a pendulum decreased with decreasing length and that the rate at which balls rolled down inclined planes increased with the angle of inclination. But he did not determine the precise quantitative relations involved. His main criticisms of the laws of motion of Aristotle and of Bradwardine were directed to pointing out their internal inconsistency, and the experiments he described were no doubt for the most part 'thought experiments.'

Better work was done in astronomy by Georg Peur-

bach (1423–61) and Johannes Müller or Regiomontanus (1436–76). Peurbach, who held a professorship at Vienna, assisted in a revision of the *Alfonsine Tables*. Perceiving, as some 14th-century writers had done, the advantage of using sines instead of chords, he computed a table of sines for every 10'. Regiomontanus, who knew the work of Levi ben Gerson (see Vol. I, p. 96), wrote a systematic treatise on trigonometry which was to have a great influence, computing a table of sines for every minute and a table of tangents for every degree. He completed a textbook begun by Peurbach and based on Greek sources, the *Epitome in Ptolemæi Almagestum*, which was printed at Venice in 1496. Another work by Peurbach, his *Theoricæ Novæ Planetarum*, published in Nuremberg in 1472 or 1473, is interesting for its diagrams of the system of solid spheres. Regiomontanus' pupil, Bernard Walther (1430–1504), with whom he collaborated in the observatory built at Nuremberg, was the first to employ for purposes of scientific measurement a clock driven by a hanging weight. In this the hour wheel was fitted with 56 teeth so that each tooth represented a fraction more than a minute.

The precise manner in which, granting the overriding importance of the conceptual revolution that accompanied the dynamics of inertia, there is continuity of historical development from the mathematical physics of the 14th century to that of the 16th and 17th, presents a delicate problem on which much scholarship has been spent. Of the basic differences in philosophical aims and methods associated with the new dynamics, changes whose establishment was the achievement of Galileo, there can be no question, as will be discussed more fully on a later page. But in comparison with 17th-century physics, that of the 14th century was also limited in both experimental and mathematical technique. The failure to put into general practice the experimental method so brilliantly initiated in the 13th century and the excessive passion for logic, which affected science as a whole, meant that the factual basis of the theoretical discussions was sometimes very slight. The mathematical expression of qualitative intensity in the 'art of latitudes,' as it was called, thus gave rise to the same

naïve excesses as the analogous attempts, to which this was the father, at omnicompetent mechanism in the 17th and 18th centuries. For instance, Oresme extended the *impetus* theory to psychology. One of his followers, Henry of Hesse (1325–97), while doubting whether the proportions and intensions of the elements in a given substance were knowable in detail, seriously considered the possibility of the generation of a plant or animal from the corpse of another species, for example of a fox from a dead dog. For although the number of permutations and combinations was enormous, during the corruption of a corpse the primary qualities might be altered to the proportions in which they occurred in some other living thing. Dumbleton and other writers had discussed at length latitudes of moral qualities like truth, faith, and perfection. Gentile da Foligno (d. 1348) applied the method to Galen's physiology and this was elaborated in the 15th century by Jacopo da Forli and others who treated health as a quality like heat and expressed it in numerical degrees. Such elaborately subtle and in practice sterile applications of a method called down the ridicule of humanists like Luis Vives (1492–1540) and Pico della Mirandola (1463–94), and made Erasmus (1467–1536) groan when he remembered the lectures he had had to endure at the university. The same geometrical ideal was expressed again in 1540 by Rheticus when he said that medicine could achieve the perfection to which Copernicus had brought astronomy, and again by Descartes.

(5) THE CONTINUITY OF MEDIEVAL AND 17TH CENTURY SCIENCE

Many scholars now agree that 15th-century humanism, which arose in Italy and spread northwards, was an interruption in the development of science. The 'revival of letters' deflected interest from matter to literary style and, in turning back to classical antiquity, its devotees affected to ignore the scientific progress of the previous three centuries. The same absurd conceit that led the humanists

to abuse and misrepresent their immediate predecessors for using Latin constructions unknown to Cicero and to put out the propaganda which, in varying degrees, has captivated historical opinion until quite recently, also allowed them to borrow from the scholastics without acknowledgment. This habit affected almost all the great scientists of the 16th and 17th centuries, whether Catholic or Protestant, and it has required the labours of a Duhem or a Thorndike or a Maier to show that their statements on matters of history cannot be accepted at their face value.

This literary movement performed some important services for science. Ultimately perhaps the greatest of these was the simplification and clarification of language, although this occurred mainly in the 17th century when it applied particularly to French, but also, under the influence of the Royal Society, to English. The most immediate service was to supply the means of developing mathematical technique. The development and physical application of the many problems discussed in Oxford, Paris, Heidelberg or Padua in terms of logic and simple geometry were sharply limited by lack of mathematics. It was unusual for medieval university students to progress beyond the first book of Euclid, and although the Hindu system was known, Roman numerals continued in use, although not among mathematicians, into the 17th century. Competent mathematicians, such as Fibonacci, Jordanus Nemorarius, Bradwardine, Oresme, Richard of Wallingford and Regiomontanus were, of course, better equipped and made original contributions to geometry, algebra and trigonometry, but there was no continuous mathematical tradition comparable with that in logic. The new translations by the humanists, presented to the public through the newly-invented printing press, placed the wealth of Greek mathematics within easy grasp. Some of these Greek authors, such as Euclid and Ptolemy, had been studied in the preceding centuries; others, such as Archimedes, Apollonius and Diophantus, were available in earlier translations but not generally studied. Among works on applied mathematics Ptolemy's *Cosmographia* and *Geographia* were both printed several times, but the *Almagest* was not printed,

except as epitomised by Regiomontanus, until early in the 16th century. Few Arabic astronomical writings were printed. By far the most editions of any author were those of Aristotle's writings, often accompanied by the glosses of Averroës and other commentators.

The whole conception of nature was affected by the systematic atomism found in the full text of Lucretius' *De Rerum Natura* discovered in a monastery in 1417 by a humanist scholar, Poggio Bracciolini. Certainly Lucretius' ideas were not unknown before this date. They appear, for example, in the writings of Hrabanus Maurus, William of Conches, and Nicholas of Autrecourt. But Lucretius' poem seems to have been known only in part, in quotations in the books of grammarians. It was printed later in the 15th century and thereafter many times.

Not only mathematics and physical science, but also biology benefited from the texts and translations published by the humanists. The humanist press made readily available the works of authors who had been either, like Celsus (*fl.* 14–37 A.D.), previously unknown or, like Theophrastus, known only through secondary sources, and new translations of Aristotle and Galen and of Hippocrates. The last came to replace Galen as the chief medical guide, greatly to the advantage of empirical practice. Pliny's *Natural History* was printed many times and Dioscorides' *De Materia Medica* twice, and there were many editions of Arabic medical writers in Latin translation: Avicenna, Rhazes, Mesue, Serapion. The new texts acted as a stimulus to the study of biology in what was at first a very curious way, for not the least important motive was the desire of humanist scholars, with their excessive adulation of antiquity, to identify animals, plants and minerals mentioned by classical authors. The limitations of this motive were eventually made evident by the very biological studies which it inspired, for these revealed the limitations of classical knowledge, and this was shown still further by the new fauna and flora discovered as a result of geographical exploration, by the increasing practical knowledge of anatomy being acquired by the surgeons, and by the brilliant advances in biological illustration stimulated by naturalistic art. But

the original humanist motive draws attention to a feature of 16th- and early 17th-century science in nearly all its branches which historians of science of an earlier generation than the present would have been inclined to associate rather with the preceding centuries; for it was just this extravagant reverence for the ancients, just this devotion to the texts of Aristotle or Galen, that provoked the sarcastic hostility of the contemporary scientists who were trying to use their eyes to look at the world in a new way. And the beginning of this new science dates from the 13th century.

The principal original contributions made during the Middle Ages to the development of natural science in Europe may be summarised as follows:

1. In the field of scientific method, the recovery of the Greek idea of theoretical explanation in science, and especially of the 'Euclidean' form of such explanation and its use in mathematical physics, raised the problems of how to construct and to verify or falsify theories. The basic conception of scientific explanation held by the medieval natural scientists came from the Greeks and was essentially the same as that of modern science. When a phenomenon had been accurately described so that its characteristics were adequately known, it was explained by relating it to a set of general principles or theories connecting all similar phenomena. The problem of the relation between theory and experiment presented by this form of scientific explanation was analysed by the scholastics in developing their methods of 'resolution and composition.' Examples of the use of the scholastic methods of induction and experiment are seen in optics and magnetics in the thirteenth and fourteenth centuries. The methods involved everyday observations as well as specially devised experiments, simple idealisations, and 'thought experiments,' but also mention of imaginary and impossible experiments.

2. Another important contribution to scientific method was the extension of mathematics to the whole of physical science, at least in principle. Aristotle had restricted the use of mathematics, in his theory of the subordination of

one science to another, by sharply distinguishing the expli-
cative roles of mathematics and 'physics.' The effect of this
change was not so much to destroy this distinction as
to change the kind of question scientists asked. One prin-
cipal reason for the change was the influence of the Neo-
platonic conception of nature as ultimately mathematical,
a conception exploited in the notion that the key to the
physical world was to be found in the study of light. Cer-
tainly the medieval scientists did not press this concep-
tion to the limit, but they did begin to show less interest
in the 'physical' or metaphysical question of cause and to
ask the kind of question that could be answered by a mathe-
matical theory within reach of experimental verification.
Examples of this method are seen in mechanics, optics and
astronomy in the 13th and 14th centuries. It was through
the mathematicisation of nature and of physics that the
inconvenient classical concept of pairs of opposites was re-
placed by the modern concept of homogeneous linear
measures.

3. Besides these ideas on method, though often closely
connected with them, a radically new approach to the
question of space and motion began at the end of the 13th
century. Greek mathematicians had constructed a mathe-
matics of rest, and important advances in statics had been
made during the 13th century, progress assisted by Archi-
medean methods of manipulating ideal quantities such as
the length of the weightless arm of a balance. The 14th
century saw the first attempts to construct a mathematics
of change and motion. Of the various elements contribut-
ing to this new dynamics and kinematics, the ideas that
space might be infinite and void, and the universe without
a centre, undermined Aristotle's cosmos with its qualita-
tively different directions and led to the idea of relative
motion. Concerning motion, the chief new idea was that
of *impetus,* and the most significant characteristic of this
concept was that a measure was given of the quantity of
impetus in which this was proportional to the quantity of
matter in the body and the velocity imparted to it. Also
important was the discussion of the persistence of *impetus*

in the absence of resistance from the medium and of the action of gravity. *Impetus* was still a 'physical' cause in the Aristotelian sense; in considering motion as a state requiring no continuous efficient causation, Ockham made another contribution perhaps related to the 17th-century idea of inertial motion. The theory of *impetus* was used to explain many different phenomena, for instance the motion of projectiles and falling bodies, bouncing balls, pendulums and the rotation of the heavens or of the earth. The possibility of the last was suggested by the concept of relative motion, and objections to it from the argument from detached bodies were met by the idea of 'compound motion' advanced by Oresme. The kinematic study of accelerated motion began also in the 14th century, and the solution of one particular problem, that of a body moving with uniform acceleration, was to be applied later to falling bodies. Discussions of the nature of a continuum and of maxima and minima began also in the 14th century.

4. In the field of technology, the Middle Ages saw some remarkable progress. Beginning with new methods of exploiting animal-, water- and wind-power, new machines were developed for a variety of purposes, often requiring considerable precision. Some technical inventions, for instance the mechanical clock and magnifying lenses, were to be used as scientific instruments. Measuring instruments such as the astrolabe and quadrant were greatly improved as a result of the demand for accurate measurement. In chemistry, the balance came into general use. Empirical advances were made and the experimental habit led to the development of special apparatus.

5. In the biological sciences, some technical advances were made. Important works were written on medicine and surgery, on the symptoms of diseases, and descriptions were given of the flora and fauna of different regions. A beginning was made with classification, and the possibility of having accurate illustrations was introduced by naturalistic art. Perhaps the most important medieval contribution to theoretical biology was the elaboration of the idea of a scale of animated nature. In geology observations were

made and the true nature of fossils understood by some writers.

6. Concerning the question of the purpose and nature of science, two medieval contributions may be singled out. The first is the idea, first explicitly expressed in the 13th century, that the purpose of science was to gain power over nature useful to man. The second is the idea insisted on by the theologians, that neither God's action nor man's speculation could be constrained within any particular system of scientific or philosophical thought. Whatever may have been its effects in other branches of thought, the effect of this idea on natural science was to bring out the relativity of all scientific theories and the fact that they might be replaced by others more successful in fulfilling the requirements of the rational and experimental methods.

Thus the experimental and mathematical methods were a growth, developing within the medieval system of scientific thought, which was to destroy from within and eventually to burst out from Aristotelian cosmology and physics. Though resistance to the destruction of the old system became strong among certain of the late scholastics, and especially among those whose humanism had given them too great a devotion to the ancient texts and those by whom the old system had been too closely linked with theological doctrines, there can be little doubt that it was the development of these experimental and mathematical methods of the 13th and 14th centuries that at least initiated the historical movement of the Scientific Revolution culminating in the 17th century.

But when all is considered, the science of Galileo, Harvey and Newton was not the same as that of Grosseteste, Albertus Magnus and Buridan. Not only were their aims sometimes subtly and sometimes obviously different and the achievements of the later science infinitely the greater; they were not in fact connected by an unbroken continuity of historical development. Towards the end of the 14th century, the brilliant period of scholastic originality came to an end. For the next century and a half all that Paris and Oxford produced on astronomy, physics, medicine or logic were dreary epitomes of the earlier writings. One or

two original thinkers like Nicholas of Cusa and Regiomon-
tanus appeared in Germany in the 15th century. Italy
fared better but rather with the new group of 'artist-
engineers' like Leonardo da Vinci than in the universities.
Interest and intellectual originality were directed towards
literature and the plastic arts rather than towards natural
science.

Apart from anything else, the enormously greater
achievements and confidence of the 17th-century scientists
make it obvious that they were not *simply* carrying on the
earlier methods though using them better. But if there is
no need to insist on the historical fact of a Scientific Revo-
lution in the 17th century, neither can there be any doubt
about the existence of an original scientific movement in
the 13th and 14th centuries. The problem concerns the
relations between them. Whatever may have happened
earlier, must the new science of the 17th century after all
be considered a completely new beginning, as some his-
torians of the past have claimed? Did the 'new philosophy,'
the 'Physico-mathematical Experimental Learning' of the
early Royal Society, spring unheralded from the heads of
Galileo and Harvey and Francis Bacon and Descartes?
Granting the great and fundamental differences between
medieval and 17th-century science, the equally striking
underlying similarities, apart from other evidence, indicate
that a more accurate view of 17th-century science is to re-
gard it as the second phase of an intellectual movement in
the West that began when the philosophers of the 13th
century read and digested in Latin translation the great
scientific authors of classical Greece and Islam.

It may be asked then what the scientists of the 16th
and 17th centuries in fact knew of the medieval work, and
how the similarities and differences of their aims may be
characterised?

As to the first question, the products of the early print-
ing presses show that the principal medieval scientific
writings were certainly made readily available, and this in
turn indicates that there was an academic demand for
them. The available data indicates, as would be expected,
that the early presses of the late 15th and early 16th cen-

turies, for example at Venice and Padua and Basel and Paris, continued to reproduce by the new process of printing the same kinds of writings that had formerly been reproduced by hand. A large proportion of these printed works were scientific, and consisted of editions of the writings of the standard classical, Arabic (in Latin translation), and medieval authors. A considerable improvement over the old manuscript copies was the publication of critical *opera omnia* in collected editions.

Although there were some notable exceptions, most of the most important medieval scientific writings were made available in print. Without going into elaborate details, these included, among the more philosophical authors, the principal writings on scientific method and philosophy of science by Grosseteste, Albertus Magnus, Aquinas, Roger Bacon, Duns Scotus, Burley, Ockham, Cusa, and the Italian Averroists from Pietro d'Abano down to Nifo and Zabarella in the early 16th century. The dynamical and kinematical writings of Bradwardine, Heytesbury, Richard Swineshead, Buridan, Albert of Saxony, and Marliani were all printed more than once, and so were some of the mathematical writings of Oresme, although not the important *De Configurationibus Intensionum* and *Livre du Ciel*. Dumbleton's writings also remained in manuscript. On statics the *Liber Jordani de Ponderibus* was published in 1533, and the *De Ratione Ponderis* of the 'school' of Jordanus Nemorarius was published by Tartaglia, in 1565. On optics the writings of Grosseteste, Roger Bacon, Witelo (together with Alhazen's treatise), Pecham, and Themon Judæi all found publishers. The most notable exception was the *De Iride* of Theodoric of Freiberg, but an account of his theory of the rainbow with the essential diagrams was published in Erfurt in 1514. Petrus Peregrinus' *Epistola de Magnete* was printed twice in the 16th century, in 1558 and 1562, and also failed to find a publisher, but was nevertheless known to and acknowledged by Gilbert. The most popular astronomical text was Sacrobosco's *Sphere*, but astronomical tables and related mathematical writings like those of Jean de Linières, Jean de Murs, Peurbach and Regiomontanus were also printed in representative quan-

tity. Chaucer's *Treatise on the Astrolabe* was printed, but Richard of Wallingford's manuscripts were not. Another very important mathematician whose writings escaped publication was Leonardo Fibonacci.

The most important medieval biologist was Albertus Magnus; his *De Animalibus* was printed and so were his geological and chemical writings. Among other printed biological works were *The Art of Falconry* of the Emperor Frederick II and the writings of Thomas of Cantimpré, Peter of Crescenzi and Conrad von Megenburg. The herbals of Rufinus and Rinio remained unprinted, but other works in this field were printed, notably Matthæus Sylvaticus' *Pandectæ,* and new herbals in Latin and in the vernacular were also issued by the presses (see below, p. 263 *et seq.*). The most popular work on natural history was Bartholomew the Englishman's *On the Properties of Things.* On anatomy, surgery and medicine the treatises, for example, of Mondino, Guy de Chauliac, Arnald of Villanova, Gentile da Foligno, and John of Gaddesden were printed many times, in some cases in several languages. Other excellent writings in this field, like those of Henri de Mondeville and Thomas of Sarepta, remained unpublished. On chemistry and alchemy the writings of Arnald of Villanova and those attributed to Raymond Lull were printed. So also were a number of practical treatises on various subjects, those of Brunschwig, Agricola and Biringuccio including much of earlier chemical practice.

The extent to which the scientists of the period showed an interest in these medieval treatises varied with different individuals. In the 16th century the strong classical leanings of men like Copernicus and Vesalius perhaps prevented them from paying much attention in print to medieval authors, but other leading scientists certainly did so. For example the Italian anatomists Achillini and Berengario da Carpi wrote commentaries on Mondino's anatomy (see below, p. 272). The theory of *impetus* and other aspects of medieval dynamics, kinematics and statics were studied and taught by mathematicians and philosophers such as Tartaglia, Cardano, Benedetti, Bonamico and the young Galileo himself. In England Dr. John Dee collected

manuscripts especially of the mathematical and physical writings of Grosseteste, Roger Bacon, Pecham, Bradwardine and Richard of Wallingford, while Robert Recorde recommended the writings of Grosseteste and other Oxford writers to students of astronomy. Dee and Recorde and Thomas and Leonard Digges were early supporters of the Copernican theory, and all saw their work as a revival of the great days of Oxford in the 13th and 14th centuries. Leonard Digges, in describing his father's pioneering work on telescopes, acknowledged Roger Bacon as an authority in optics. Leonardo da Vinci, Maurolyco, Marc Antonio de Dominis, Giambattista della Porta, Johann Marcus Marci and Christopher Scheiner all referred in their optical writings to Roger Bacon, Witelo and Pecham. Kepler wrote a commentary on Witelo, correcting his tables of angles of refraction; Snell's work on the law of refraction seems to have been stimulated by the edition of Witelo and Alhazen by Frederick Risner in 1572; and many other 17th-century optical writers, for example Descartes himself, Fermat, James Gregory, Emanuel Maignan and Grimaldi used the same source. As for Descartes, he seldom mentioned those to whom he was indebted, but his *Météores* follows the exact order of the subjects of Aristotle's *Meteorology* and is in more ways than one the last of the medieval commentaries on that much glossed work (cf. below, pp. 251–55).

Enough has been said to show that leading scientists of the 16th and early 17th centuries both knew and used the writings of their medieval predecessors. The story is the same in biology as in physics, where Albertus Magnus was the principal medieval writer. In the conceptions of scientific method and explanation the medieval part of the ancestry is equally visible, especially for example in Galileo's use of the methods of 'resolution and composition' to elucidate the relation between theory and experiment and to develop the 'Euclidean' form of scientific explanations. So it is also in the Neoplatonic conception of nature as ultimately mathematical, first exploited in the Middle Ages in Grosseteste's 'cosmology of light' and apparent in different ways in the thought of Galileo, Kepler and Descartes. But did the scientists, especially of the 17th century, sim-

ply accept and continue the aims and methods of the scholastics? It will appear in greater detail in the chapter that follows that clearly they did much more. One characteristic may be singled out as indicating an essential difference.

The central doctrines of medieval science developed almost entirely within the context of academic discussions based at some stage, near or far, on the books used in university teaching. The commentaries and *quæstiones* on the subjects treated in these books may have travelled far from the originals of Aristotle or Ptolemy or Euclid or Alhazen or Galen; they never escaped from them altogether. It is true that the applications of academic sciences, such as of astronomy in determining the calendar and making proposals for its reform, or of arithmetic in the work of the exchequer and of commercial houses, or of anatomy and physiology and chemistry in surgery and medicine, were put into practice outside the universities. It is true also that in other fields outside the university system altogether, for example in technology of different kinds and in art and architecture with their increasing tendency to naturalism, developments took place that were to be of profound importance for science. Certainly the reasons for the development of science within the universities, and for the growth and spread of the university system itself, must be related to the reasons for the development of national political states based on an expanding commercial capitalism that could give employment to the men responsible for these technological and artistic activities outside. The latter, becoming the 'artist-engineers' of the 15th and 16th centuries and the *virtuosi* and independent scientific gentlemen of the 17th, were to take over the leadership of science, making it more an activity of the Accademia dei Lincei or the Royal Society or the Académie Royale des Sciences than of the universities. This was true even though in these scientific societies there was a predominance of university men, who were in fact to bring the new science back into the universities themselves.

But in the 13th and 14th centuries it was within the framework of the university faculty of arts, its curriculum

expanded to include the new translations from Greek and Arabic and some technical treatises on applied mathematics, and of the higher faculties of medicine and of theology, that the central conceptions of science were cultivated. The men who cultivated them were clerics and academic teachers. The academic exercise was never far away in the background of the treatises they have left behind, those unliterary writings that form the great collections of manuscripts and early printed books that show us their ways of thought. Certainly many of them were original and ingenious thinkers. But the great scientific and cosmological problems with which they dealt were seldom seen by them as purely scientific. The greatest problem of all was the relation of the cosmology of Christian theology based on revelation to the cosmology of rational science dominated by Aristotle's philosophy. Although some of the best medieval scientific work was done on particular problems studied without any reference to theology or philosophy or even methodology, it was within a general framework of philosophy closely bearing on theology, and specifically within the system of university studies run by clerics, that the central development of medieval science took place.

The result of this was that science in the Middle Ages was nearly always at the same time philosophy of science. No doubt the same characteristics will appear in any age that is still determining the direction and objectives of its inquiries, as they did eminently in the 17th century, for example in the scientific thought and controversies of Galileo, Descartes and Newton. In contrast with both medieval and 17th-century scientists, those of the 20th century know in general how they are going to deal with problems, the kinds of questions they are going to put to nature and the methods they will employ to get their answers. It is only in the profoundest and most general problems, when a line of explanation seems to meet with an *impasse*, that philosophy need nowadays disturb the even course of the bulk of the scientific work actually being done.

But there is one basic difference between the aims of medieval philosophy of science and of all the philosophy of

science since Galileo. The latter is *primarily* concerned with clarifying and facilitating the processes and further advances of science itself. The main interest of scientists since Galileo has been in the ever-increasing range of concrete problems that science can solve, and if philosophical investigations are undertaken by scientists, it is usually because certain concrete and specific scientific problems can be satisfactorily solved only by a thorough reform of fundamental principles. The essays in philosophy by Galileo and Newton had essentially this purpose. But medieval natural philosophers were *primarily* interested less in the concrete problems of the world of experience than in the *kind of knowledge* natural science was, how it fitted into the general structure of their metaphysics, and, if it extended so far, how it bore on theology. Many scientific problems were discovered as analogies that could illuminate a theological problem, as was the case with instrumental causality and the theory of *impetus*. Being taken up in the interests of something else, this was no doubt one reason why in the course of development they were so often so peremptorily dropped.

The contrast is one of general emphasis and is certainly not exclusive. In the 18th century Berkeley and Kant, for example, were primarily concerned not with science but with the bearing of Newtonian cosmology on metaphysics, while in the 13th century Jordanus, Gerard of Brussels and Petrus Peregrinus seem to have been innocent of any philosophical interests and purely concerned with the immediate scientific problems in hand. But if what has been said does truly characterise the general intellectual ambience of medieval science, it explains much that is puzzling and seemingly downright perverse in otherwise excellent work. It helps to explain, for example, the gap between the repeated insistence on the principle of empirical verification and the many general assertions never tested by observation; worse, the satisfaction with imaginary experiments either incorrect or impossible; even worse, the false figures given, for example, by scientists of the calibre of Witelo or Theodoric of Freiberg allegedly as the results of measurements plainly never made. There are of course

examples of medieval science not marred by such defects, but it was a peculiarity of the period that they could occur in the course of even the best-conceived investigations. The impression is left that the investigator was not strongly interested in mere details of fact and measurement. Certainly the strong interest in the theory and logic of experimental science and in related philosophical conceptions of nature, sustained from Grosseteste down to the threshold of Galileo's activities, stands in striking contrast with the comparative scarcity of actual experimental investigations. This becomes intelligible if we see the medieval natural philosophers not as modern scientists *manqués* but as primarily philosophers. They gave an account of experimental inquiries often as an exercise in what could be done in one branch of philosophy in distinction from others. Certainly this had the desirable effect of clarifying the problems of natural science and helping to extricate them from alien contexts of metaphysics and theology. In what was actually found out by experiment they were less interested.

It was a direction of interest that could have been fatal to Western science. Excellent as may have been much of their general characterisation of the methodology of experimental science, it meant that the methodologists seldom really put their methods to the practical test. So they rarely made them really precise or really adequate. Undirected experiments and simple everyday observations abound in the work of medieval scientists. Certainly there was no general movement to conceive of experimental inquiry as a sustained testing of a series of precisely and quantitatively formulated hypotheses, pressing on to the reformulation of a whole area of theory. The examples of experimental inquiries, even the best of them, remained isolated without general effect on the accepted doctrines of light or of cosmology. They were thought sufficient to illustrate the method, and methodology was an end in itself. It would have become a dead end had not Galileo and his contemporaries, with a new direction of interest, pursued the subjects of the examples for their own sakes. It was through taking these seriously, through paying attention to the detailed facts of experiment and measurement and mathe-

matical functions actually exemplified in nature, that the
17th-century scientists were led to their radical revolution
in the whole theoretical framework of physics and cosmol-
ogy, where the medieval natural philosophers had only
revised some limited sections.

If it is true that a fundamental change in the interests
of scientists and in the conception of science can be charted
about the time of Galileo, a further point would indicate
another detail of the general line of change. Perhaps the
most powerful feature of the medieval philosophy of sci-
ence that remained strongly influential in the early 17th
century was the Neoplatonic conception that nature was
ultimately to be explained by mathematics. In the Middle
Ages this belief was exploited mainly in the field of optics.
Within the ambience of Platonism, and encouraged by the
story in *Genesis* of the first day of creation, leading thinkers
of the 13th and 14th centuries focussed their attention on
the study of light as the key to the mysteries of the physical
world, and in optics they did some of their best scientific
work. But, as in the Aristotelian classification, optics re-
mained, together with astronomy and music, one of the
mathematica media, mathematical sciences applied to the
physical world as distinct on the one hand from pure math-
ematics, and on the other from physics as the science of
'natures' and causes. Medieval scientists seemed to feel no
overwhelming desire or need to dispense with these philo-
sophical distinctions. Mathematical physics never really be-
came a universal science rendering Aristotelian physics
unnecessary.

Perhaps it was pointed of Descartes, the most medieval
of the great 17th-century scientists in the sense of being
the most dominated by a philosophy of nature, to call his
reforming work on cosmology *Le Monde, ou Traité de la
Lumière*. But Descartes' physics were not based on a theory
of light; rather his theory of light was based on his con-
ception of motion. It was in the study of motion and not of
light that the 17th-century scientists looked for the key to
physics. It was there too that to their satisfaction they
found it.

Certainly in giving special weight to the study of motion

as distinct from other aspects of nature the 17th-century physicists made a fortunate choice. But Aristotle and the medieval Aristotelians had already made the study of motion the basis of their physics. The choice made by the 17th-century scientists was not fortuitous, nor was the success with which it was exploited. By taking the empirical phenomena of motion seriously as a problem and seeing the solution through to the end, they had no alternative but to reform the whole of cosmology, to invent new mathematical techniques in the process, and to provide the eminent example for the methods of science as a whole. This, it may be suggested, was the advance made by the secular *virtuosi* of the 17th century over the clerics of the medieval universities to whom in other ways they owed so much.

II

THE REVOLUTION IN SCIENTIFIC THOUGHT IN THE SIXTEENTH AND SEVENTEENTH CENTURIES

(1) THE APPLICATION OF MATHE-MATICAL METHODS TO MECHANICS

How the scientific revolution of the 16th and 17th centuries came about is easier to understand than the reason why it should have taken place at all. So far as the internal history of science is concerned, it came about by men asking questions within the range of an experimental answer, by limiting their inquiries to physical rather than metaphysical problems, concentrating their attention on accurate observation of the kinds of things there are in the natural world and the correlation of the behaviour of one with another rather than on their intrinsic natures, on proximate causes rather than substantial forms, and in particular on those aspects of the physical world which could be expressed in terms of mathematics. Those characteristics that could be weighed and measured could be compared, could be expressed as a length or number and thus represented in a ready-made system of geometry, arithmetic or algebra, in which consequences could be deduced revealing new relations between events which could then be verified by observation. The other aspects of matter were ignored.

The systematic use of the experimental method by

which phenomena could be studied under simplified and controlled conditions, and of mathematical abstraction which made possible new classifications of experience and the discovery of new causal laws, enormously speeded up the tempo of scientific progress. One outstanding fact about the Scientific Revolution is that its initial and in a sense most important stages were carried through before the invention of the new measuring instruments, the telescope and microscope, thermometer and accurate clock, which were later to become indispensable for getting the accurate and satisfactory answers to the questions that were to come to the forefront of science. In its initial stages, in fact, the Scientific Revolution came about rather by a systematic change in intellectual outlook, in the type of question asked, than by an increase in technical equipment. Why such a revolution in methods of thought should have taken place is obscure. It was not simply a continuation of the increasing attention to observation and to the experimental and mathematical methods that had been going on since the 13th century, because the change took on an altogether new speed and a quality that made it dominate European thinking. It is not an adequate explanation to say that the new approach was simply the result of the work done on inductive logic and mathematical philosophy by the scholastic philosophers down to the 16th century or the result of the revival of Platonism in the 15th century. It cannot be attributed simply to the effect of the renewed interest in some hitherto poorly known Greek scientific texts, such as the work of Archimedes, though these certainly stimulated mathematical thought.

Various aspects of the social and economic conditions of the 16th and 17th centuries certainly provided motives and opportunities that might stimulate science. At the beginning of the 16th century some outstanding scholars showed a vigorous interest in the study of the technical processes of manufacture, and this helped to unite the mind of the philosopher with the manual skill of the craftsman. Luis Vives wrote in 1531 in his *De Tradendis Disciplinis* advocating the serious study of the arts of cooking, building, navigation, agriculture and clothmaking, and

specifically urged that scholars should not look down on manual workers or be ashamed of asking them to explain the mysteries of their crafts. Rabelais, writing two years later, suggested that a proper branch of study for a young prince was to learn how the objects he used in ordinary life were made. Rabelais described how Gargantua and his tutor visited goldsmiths and jewellers, watchmakers, alchemists, coiners and many other craftsmen. In 1568 a Latin reader published in Frankfort for the use of school children seems to have been inspired by the same respect for skilled craftsmanship, for it took the form of a series of Latin verses each describing the work of a different craftsman, for example a printer, a papermaker, a pewterer or a turner. A marked advance was made during the 16th century also in the publication of treatises written by the educated on various technical processes. Of these, *De Re Metallica* (1556) by Georg Bauer (1490–1555), or Agricola, as he called himself, on mining and metallurgy and the treatises by Besson, Biringuccio, Ramelli and, in the early 17th century, by Zonca are the most outstanding examples (cf. Vol. I, pp. 176–78). This interest in the technical achievements of the various crafts was expressed most clearly by Francis Bacon (1561-1626), first in 1605 in *The Advancement of Learning* and later more fully in the *Novum Organum*. Bacon was of the opinion that technics or, as he called it, the mechanical arts, had flourished just because they were firmly founded on fact and modified in the light of experience. Scientific thought, on the other hand, had failed to advance just because it was divorced from nature and kept remote from practical experiment In his view the learning of the schoolmen had been 'cobwebs of learning . . of no substance or profit' and the new humanistic learning must be directed to the benefit of man. Descartes took precisely the same view of the matter. In the 16th century several mathematicians such as Thomas Hood (*fl.* 1582–98) and Simon Stevin (1548-1620) were specially employed by governments to solve problems of navigation or fortification. In the latter part of the 17th century the Royal Society interested itself in the technical processes of various trades in the hope that

the information collected would not only provide a solid foundation for the speculations of scholars, but also would be of practical value to mechanics and artificers themselves. Several treatises were collected on special subjects: Evelyn wrote a *Discourse of Forest-Trees and the Propagation of Timber,* Petty on dyeing, and Boyle a general essay entitled *That the Goods of Mankind may be much increased by the Naturalist's Insight into Trades.* The English History of Trades did not get written, but the idea was attractive and almost a century later twenty volumes on arts and crafts were published by the Paris Academy of Sciences.

There are also, certainly, examples of this active interest of the learned in technical questions leading scientists to make contributions to fundamental problems. The attempt to calculate the angle at which a gun must be fired to give the maximum range led Tartaglia (*c.* 1500-57) to criticise the whole Aristotelian conception of motion and attempt new mathematical formulations, though the problem was solved only by Galileo. The experience of engineers who built water pumps is said to have influenced Galileo and Torricelli in their experiments leading to the barometer, and the rumour that some Dutch lens-grinders had invented a telescope is known to have stimulated Galileo to study the laws of refraction with the object of constructing one himself. Descartes wrote his *Dioptrique* (1637) explicitly to give a scientific basis for constructing lenses for telescopes and spectacles. When they did their fundamental work on the pendulum, both Galileo and Huygens had in mind the need for an accurate clock for determining longitude, which became increasingly pressing with the extension of ocean voyages.

The existence of motives and opportunities, even when they brought fundamental scientific problems into prominence, does not explain the intellectual revolution that made it possible for scientists to solve these problems, and the history of the interaction between motives, opportunities, skills and intellectual changes that brought about the Scientific Revolution has, in fact, yet to be written.

The internal revolution in scientific thought that took

place during the 16th and 17th centuries had, then, two essential aspects, the experimental and the mathematical, and it was precisely those branches of science which were most amenable to measurement that showed the most spectacular developments. In antiquity mathematics had been used most successfully in astronomy, optics and statics, and to these the medieval schoolmen, less successfully, added dynamics. These were also the branches of science which showed the greatest advances in the 16th and 17th centuries and, in particular, it was the successful application of mathematics to mechanics that changed men's whole conception of nature and brought about the destruction of the whole Aristotelian system of cosmology. It was only after they had, following the Greek example, successfully applied their new methods to these abstract and comparatively manageable problems, that scientists found themselves in a position to attack the more difficult mysteries of dead and living matter. Chemistry, physiology, and the sciences of electricity and magnetism cannot until the 19th century compare in performance with Newtonian mechanics (cf. above, p. 8, below, p. 323).

One of the first to try to express nature in terms of the new mathematics was Leonardo da Vinci (1452–1519). Leonardo received his early education in the Platonic city of Florence and later worked in Milan and the other northern Italian towns where the scientific ideal was Aristotelian. Nearly all his physical conceptions were suggested by scholastic writers, such as Jordanus Nemorarius, Albert of Saxony and Marliani, but he was able to develop their mechanical ideas through his new knowledge of Greek mathematicians like Archimedes, to whose *On the Equilibrium of Planes* he had access in manuscript.

Among ancient mathematicians Archimedes had been the most successful in combining mathematics with experimental inquiry; because of this he became the ideal of the 16th century. His method was to select definite and limited problems, and it would be truer to say that he proceeded rather by the mathematical manipulation of ideal quantities than by actual measurement. He formulated hypotheses which he either regarded, in the Euclidean

manner, as self-evident axioms or could verify by simple experiments. The consequences of these he then deduced and, in principle, experimentally verified. Thus, in the work just mentioned, he began with the axioms that equal weights suspended at equal distances are in equilibrium, that equal weights suspended at unequal distances are not in equilibrium but that which hangs at the greater distance descends, and so on. These axioms contained the principle of the lever, or, what is equivalent, of the centre of gravity, and from them Archimedes deduced numerous consequences.

Leonardo's mechanics, like those of his predecessors, was based on Aristotle's axiom that motive power is proportional to the weight of the body moved and the velocity impressed on it. Jordanus Nemorarius and his school had developed this axiom to express the principle of virtual velocity or work, and applied it, with the notion of the statical moment, to the lever and the inclined plane. Leonardo used the conclusions of this school and made various advances on them. He recognised that the effective (or potential) arm of a balance was the line which, passing through the fulcrum, was at right angles to the perpendicular passing through the suspended weight. He recognised that a sphere on an inclined plane moved until it reached a point where its centre of gravity was vertically above its point of contact, though he rejected Jordanus' correct treatment of equilibrium on an inclined plane for an incorrect solution given by Pappus. He did recognise that the velocity of a ball rolling down an inclined plane was uniformly accelerated, and showed that the velocity of a falling body increased by the same amount for a given vertical fall whether it descended vertically or down an incline. He also recognised that only the vertical component need be considered in estimating motive power, and that the principle of work was incompatible with perpetual motion: he said that if a wheel were moved for a time by a given quantity of water and if this water were neither added to nor allowed a greater fall, then its function was finished. The principle of work, with that of the lever, he used also to develop the theory of pulleys and other mechanical appli-

ances. In hydrostatics he recognised the fundamental principles that liquids transmit pressure and that the work done by the mover equals that done by the resistance. In hydrodynamics he developed the principle which the school of Jordanus had derived from Strato, that, with a given fall, the smaller the cross-section of the passage the greater the velocity of a flowing liquid.

Leonardo's dynamics was based on the theory of *impetus*, which, he held, carried the moving body in a straight line. But he adhered (like Cardano, Tartaglia and other later 16th-century Italian mechanicians) to the Aristotelian view that the supposed acceleration of a projectile after leaving the projector was due to the air. He accepted also Albert of Saxony's division of the trajectory of a projectile into three periods, but he recognised that the actual motion of a body might be the resultant of two or more different forces or velocities. He applied the principle of compound *impetus*, together with that of centre of gravity which he derived from Albert of Saxony and developed for solid figures, to a number of problems including percussion and the flight of birds.

In addition to his studies in mechanics Leonardo also used Greek geometry in an attempt to improve the theory of lenses and the eye which he derived from an edition of Pecham's *Perspectiva Communis*, printed in 1482. He made certain advances but suffered, like his predecessors, from the belief that the visual function resides in the lens instead of the retina, and from the inability to understand that an inverted image on the latter was compatible with seeing the world in the way we do. His devotion to the ideal of measurement is shown by the scientific instruments which he tried to improve or devise, such as a clock, a hygrometer similar to Cusa's to measure moisture in the atmosphere, a hodometer similar to Hero's to measure distance travelled, and an anemometer to measure the force of the wind. Though he wrote no book and his illegible mirror-written notes covered with sketches were not deciphered and published until much later, many of them not until the 19th century, his work was not lost to his immediate posterity. His manuscripts were copied in the

16th century and his mechanical ideas were pillaged by Hieronymo Cardano (1501–76), and may have passed to Stevin and through Bernardino Baldi to Galileo, Roberval and Descartes. The Spaniard Juan Batiste Villalpando (1552–1608) made use of his ideas on the centre of gravity, and from him they were transmitted, through the wide scientific correspondence of the learned Minim friar, Marin Mersenne, to the 17th century.

The natural philosophers who succeeded Leonardo developed still further the powerful mathematical technique that was becoming possible with the recovery and printing of some hitherto unknown or little studied Greek texts. The earliest printed Latin edition of Euclid appeared in Venice in 1482, and Latin editions of Archimedes, Apollonius and Diophantus were made by Francesco Maurolyco (1494–1575) and of Euclid, Apollonius, Pappus, Hero, Archimedes and Aristarchus by Federigo Commandino (1509–75).

The first advances in mathematical technique were in algebra. The first comprehensive printed *Algebra*, that of Luca Pacioli (1494), contained the problem of cubic equations (those involving cubes of numbers, e.g., x^3), which were first solved by Tartaglia (whose real name was Nicolo Fontana of Brescia). His work was pirated by Hieronymo Cardano, who anticipated him in publication (1545). Cardano's former servant and pupil, Lodovico Ferrari (1522–65), first solved quartic equations (involving x^4). Limitations in the general theory of numbers prevented the understanding of quintics (involving x^5) until the 19th century, but François Viète (1540–1603) gave a method of obtaining numerical values of the roots of polynomials and introduced the principle of reduction. The theory of equations was also developed by the English mathematician, Thomas Harriot (1560–1621). To the earlier algebraists negative roots had seemed unintelligible. These were first understood by Albert Girard (1595–1632), who also extended the idea of number to include 'imaginary' quantities like $\sqrt{-1}$, which had no place in the ordinary numerical scale extending from zero to infinity in both the negative and positive directions. At the same time

improvements were made in algebraic symbolism. Viète used letters for unknowns and constants as an essential part of algebra. Stevin invented the present mode of designating powers and introduced fractional exponents. His symbolism was later generalised by Descartes in the form x^2, x^3, etc. Other symbols such as $+$, $-$, $=$, $>$, $<$, V, etc., to represent operations which had previously been written out in words, had been gradually introduced from the end of the 15th century, so that by the first decades of the 17th century algebra and arithmetic had been standardised into something like their present form.

About the same time two important advances were also made in geometry. The first was the introduction of analytical geometry, the second the emergence of infinitesimal calculus. A step towards analytical geometry had been made by Nicole Oresme and there are reasons for believing that Descartes, who was not in the habit of mentioning those to whom he was indebted, knew his work. The man to whom Descartes was probably most indebted here was Pierre de Fermat (1601–65), who fully grasped the equivalence of different algebraic expressions and geometrical figures traced by loci moving with reference to co-ordinates. If his predecessors invented the method, it was Descartes who, in his *Géométrie* (1637), first developed its full power. He rejected the dimensional limitation on algebra and by letting, for instance, squares or cubes of terms (x^2, y^3) represent lines, he was able to put geometrical problems into algebraic form and to use algebra to solve them. Problems of motion thus received fruitful development when a curve could be represented as an equation (see Pl. III). Descartes also showed that the entire conic sections of Apollonius were contained in some equations of the second degree.

Descartes' analytical geometry depended on the assumption that a length was equivalent to a number; this no Greek would have accepted. The second mathematical advance made during the early years of the 17th century depended on a similar pragmatic illogicality. To compare rectilinear and curvilinear figures, Archimedes had used the 'method of exhaustion.' In this the area of a curvi-

linear figure could be determined from that of inscribed
and circumscribed rectilinear figures, by making them ap-
proach the curve by increasing the number of their sides.
When determining elliptical areas Kepler had introduced
the idea of the infinitely small into geometry and Fran-
cesco Bonaventura Cavalieri (1598–1647) made use of this
idea to develop Archimedes' method into the 'method of
indivisibles.' This depended on considering lines as com-
posed of an infinite number of points, surfaces of lines
and volumes of surfaces. The relative magnitudes of two
surfaces or solids could then be found simply by summa-
tion of series for points or lines. In contrast with Descartes'
analytical geometry, which was not generally used in phys-
ics until the end of the 17th century, the 'method of in-
divisibles' arose directly out of physical problems. It was
later developed by Newton and Leibniz into the infinitesi-
mal calculus.

Aristotle had maintained, as against the Pythagorean
theory of Plato, that mathematics, though useful in defin-
ing the relations between certain events, could not express
the 'essential nature' of physical things and processes, for
it was an abstraction excluding from consideration irreduci-
ble qualitative differences which, nevertheless, existed. Ac-
cording to Aristotle, the study of physical bodies and
events was the proper object not of mathematics but of
physics. In studying them, he arrived at such essential
distinctions not only as those between irreducibly different
qualities perceived through the senses but also, in the con-
sideration of observed motions, as those between natural
and violent movements, gravity and levity, and terrestrial
and celestial substance. This point of view had been
shared by Euclid and was accepted by Tartaglia in his com-
mentary on the *Elements*. Tartaglia said that the subject-
matter of physics, which was gained through sensory expe-
rience, was distinct from the subject-matter of geometrical
demonstration. A physical speck, for instance, was divisible
to infinity, but a geometrical point, being without dimen-
sions, was by definition indivisible. The subject-matter of
geometry, he said, was continuous quantity-point, line,
volume, and its definitions were purely operational. Ge-

ometry was not concerned with what exists; it could deal
with physical properties like weight or time only when
these had been translated into lengths by measuring in-
struments. Since its principles were known by abstraction
from material things, the conclusions it demonstrated were
applicable to them. Thus physics might use mathematics,
but was left with an independent non-mathematical field
of its own.

With the increasing success of mathematics in solving
concrete physical problems during the 16th century, the
area of this purely physical preserve was reduced. The
practical geometers of the 16th century developed the idea
of using measurements, for which instruments of increas-
ing accuracy were required, to determine whether what
held true in mathematical demonstrations also held true
in physical things. For instance, Tartaglia accepted the
Aristotelian principle, which had led to a three-fold divi-
sion of the trajectory of a projectile (cf. Fig. 1), that an
elementary body could have only a single movement at
any time (since if it had two one would eliminate the
other). When he came to make a mathematical study of
the flight of a projectile, he realised that when fired out
of the vertical it began its descent under the action of
gravity *immediately* after leaving the gun (cf. Pl. IV). He
had to maintain, therefore, that natural gravity was not
entirely eliminated by *impetus*. Cardano (who also devel-
oped Leonardo's ideas on the balance and virtual veloci-
ties) went a step further. He drew a distinction in
mechanics between mathematical relations and moving
powers or principles, the proper subject of 'metaphysics,'
and accepted the old forms of such powers. He objected
altogether to the arbitrary separation of mathematical
subject-matter into irreducibly different classes, such as in
the different periods in the trajectory of a projectile. Viète
took the same view.

The old problem of projectiles had, in fact, gained a new
importance in the 16th century when improved types of
bronze cannon with accurately bored barrels began to re-
place the 14th- and 15th-century cast-iron monsters, and
when a more powerful kind of gunpowder was produced

in Germany. At the same time improvements were made in small arms, particularly in methods of firing, and from the end of the 15th century the old method of touching off the powder by applying a burning match to the touch hole was replaced by a number of improved devices. First came the match-lock which enabled the burning match to be brought down by pressing a trigger. This was applied to the arquebus, the common infantryman's weapon after the battle of Pavia, in 1525. Then came the wheel-lock using pyrites instead of a burning match, though this was too dangerous to be much used. Finally, by 1635, came a device using flint which became the flint-lock used by the soldiers of Marlborough and Wellington. Problems of theoretical ballistics did not arise in the use of small arms, but with the heavy guns, as the range increased with more powerful gunpowder, problems of sighting became serious. Tartaglia devoted much time to these problems and the invention of the gunner's quadrant has been attributed to him. Later, Galileo, Newton and Euler made further contributions, though it was not until the second half of the 19th century that accurate ballistic tables were constructed on the basis of experiments.

Another 16th-century mathematician and physicist who made a critical scrutiny of Aristotelian theories and exposed some of their contradictions, even as a system of physics, was Giovanni Battista Benedetti (1530–90). He knew of the criticisms that had been made in late Greek times of Aristotle's ideas on falling bodies (see above, p. 50 *et seq.*). He imagined a group of bodies of the same material and weight falling beside each other, first connected and then separately, and he concluded that their being in connection could not alter their velocity. A body the size of the whole group would, therefore, fall with the same velocity as each of its components. He therefore concluded that all bodies of the same material (or 'nature'), whatever their size, would fall with the same velocity, though he made the mistake of believing that the velocities of bodies of the same volume but different material would be proportional to their weights. Inspired by Archimedes, he thought of weight as proportional to the relative density

in a given medium.[1] He then used the same argument as
Philoponus to prove that velocity would not be infinite in
a void (see above, pp. 51, 60). Benedetti also held that
in a projectile natural gravity was not entirely eliminated
by the *impetus* of flight, and he followed Leonardo in
maintaining that *impetus* engendered movement only in
a straight line, from which it might be deflected by a force,
such as the 'centripetal' force exerted by a string which
prevented a stone swung in a circle flying off at a tangent.

Sixteenth-century physicists turned increasingly from
Aristotle's qualitative 'physical' explanations to the mathe-
matical formulations of Archimedes and to the experi-
mental method. Although their enunciations were not
always rigorous, their instincts were usually sound. Like
Archimedes, they tried to conceive of a clear hypothesis
and put it to the test of experience. Thus, beginning with
the assumption that perpetual motion was impossible,
Simon Stevin was led to a clear appreciation of the basic
principles of both hydrostatics and statics. In the former
science he concluded (1586) that any given mass of water
was in equilibrium in all its parts, for if it were not it
would be in continuous movement, and he then used this
theory to show that the pressure of a liquid on the base
of the containing vessel depended only on depth and was
independent of shape and volume. Equipotential points
were those on the same horizontal surface.

With the same assumption of the impossibility of per-
petual motion he showed also why a loop of cord, on which
weights were attached at equal distances apart, would not
move when hung over a triangular prism (Fig. 3). He
showed that as long as the bottom of the prism was hori-
zontal no movement occurred in the upper section of the
cord when the suspended section was removed, and from
this he arrived at the conclusion that weights on inclined
planes were in equilibrium when proportional to the
lengths of their supporting planes cut by the horizontal.
The same conclusion had, in fact, been reached in the 13th

[1] Archimedes' principle asserts that when a body floats its weight
is equal to the weight of the liquid displaced and when it sinks
its weight decreases by that amount.
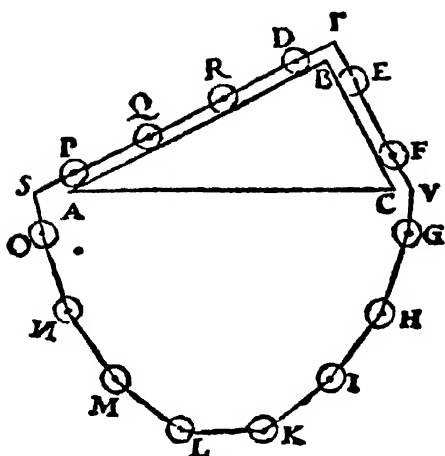
FIG. 3 *Stevin's demonstration of the equilibrium of the inclined plane. From* Beghinselen des Waterwichts, *Leyden, 1586.*

century in *De Ratione Ponderis*, which had been published in 1565 (see Vol. I, pp. 119–20). This conclusion implied the idea of the triangle or parallelogram of forces, which Stevin applied to more complicated machines.

An important statical principle arising out of this work of Stevin, though the germ of it came from Albert of Saxony, seems to have been taught by Galileo Galilei (1564–1642). This was that a set of connected bodies, such as those on Stevin's inclined plane, could not set themselves in motion unless this resulted in the approach of their common centre of gravity towards the centre of the earth. The work done was then equal to the product of the weight moved multiplied by the vertical distance. The precise enunciation of this principle and its fruitful application to mathematical physics was made by Galileo's pupil, Torricelli.

Stevin performed the experiment, also attributed to Galileo, of dropping simultaneously two leaden balls, one ten times heavier than the other, from a 30-foot height

on to a plank. They struck the plank at the same instant and he asserted that the same held for bodies of equal size but different weight, that is, of different material. Similar experiments had, in fact, been mentioned in the writings of critics of Aristotle since Philoponus, though the result was not always the same because of the appreciable effect of air resistance on the lighter bodies. Stevin and his predecessors recognised that their observations were incompatible with the Aristotelian law of motion, according to which velocity should be directly proportional to the moving cause, with falling bodies their weight, and inversely proportional to air resistance. But Stevin did not develop the dynamical consequences of his observations.

It was, in fact, Galileo who was chiefly responsible for carrying the experimental and mathematical methods into the whole field of physics and for bringing about the intellectual revolution by which first dynamics, and then all science, were established in the direction from which there was no return. The revolution in dynamics in the 17th century was brought about by the substitution of the concept of inertia, that is, that uniform motion in a straight line is simply a state of a body and is equivalent to rest, for the Aristotelian conception of motion as a process of becoming which required for its maintenance continuous efficient causation. The problem of the persistence of motion was brought to the fore because it was this Aristotelian conception that lay behind some of the most important objections to Copernicus' theory of the earth's rotation, for instance that based on the argument from detached bodies (see above, p. 76, below. p. 175), and the question of the truth of the Copernican theory was perhaps the chief scientific problem of the late 16th and early 17th centuries. To prove this theory was the great passion of Galileo's scientific life. To do so Galileo tried to ignore the naïve inductions from common-sense experience, which were the basis of Aristotle's physics, and to look at things in a new way.

Galileo's new way of looking at the facts of experience represented a change of emphasis which was all important, though each of its two main characteristics had antecedents

in an earlier tradition; the proof of it was that it bore fruit
in the rapid solution of many different scientific problems.
First, he put aside all discussion of the 'essential natures'
that had been the subject-matter of Aristotelian physics
and concentrated on describing what he observed, that is,
on the phenomena. This is seen in his *Dialogue Concern-
ing the Two Principal Systems of the World* (1632) when,
during the Second Day, Salviati, representing Galileo him-
self, replies as follows to the assertion made by Simplicio,
the Aristotelian, that everyone knows that what causes
bodies to fall downwards is gravity:

> You are wrong, Simplicio; you should say that everyone
> knows that it is called gravity. But I am not asking you
> for the name, but the essence of the thing. Of this you
> know not a bit more than you know the essence of the
> mover of the stars in gyration. I except the name that
> has been attached to the former and made familiar and
> domestic by the many experiences we have of it a thou-
> sand times a day. We don't really understand what prin-
> ciple or what power it is that moves a stone downwards,
> any more than we understand what moves it upwards
> after it has left the projector, or what moves the moon
> round. We have merely, as I said, assigned to the first
> the more specific and definite name *gravity*, whereas to
> the second we assign the more general term impressed
> power (*virtù impressa*), and the last we call an *intel-
> ligence*, either *assisting* or *informing*; and as the cause
> of infinite other motions we give *nature*.

This attitude to such so-called causes Galileo learnt from
the nominalism which had penetrated the Averroïst schools
of northern Italy during the 15th century. Such words as
'gravity,' he held, were simply names for certain observed
regularities, and the first business of science was not to
seek unfindable 'essences' but to establish these regulari-
ties, to discover proximate causes, that is, those antecedent
events which, when other conditions were the same, al-
ways and alone produced the given effect. 'Consider what
there is that is new in the steelyard,' declared Salviati in
the Second Day of the *Two Principal Systems*, 'and therein

lies necessarily the cause of the new effect.' He continued, in the Fourth Day, enunciating what J. S. Mill was to call the method of concomitant variations[2] "Thus I say that if it is true that one effect can have only one basic cause, and if between the cause and the effect there is a fixed and constant connection, then whenever a fixed and constant variation is seen in the effect, there must be a fixed and constant variation in the cause. Now since the variations which take place in the tides at different times of the year and of the month have their fixed and constant periods, it must be that regular changes occur simultaneously in the primary cause of the tides. Next, the alterations in the tides at the said times consist of nothing more than changes in their sizes; that is, in the rising and lowering of the water a greater or less amount, and in its running with greater or less impetus. Hence it is necessary that, whatever the primary cause of the tides is, it should increase or decrease its force at the specific times mentioned. . . . If then we wish to preserve the identity of the cause, we must find the changes in these additions and subtractions that make them more or less potent at producing those effects that depend upon them.'

As this passage indicates, Galileo's whole method presupposed measurement. He gave another, more qualitative, illustration of it in his witty reply in *Il Saggiatore*, question 45:

> If Sarsi wishes me to believe, on the word of Suidas, that the Babylonians cooked eggs by whirling them swiftly in a sling, I will believe it; but I shall say that the cause of such an effect is very remote from that to which they attribute it, and to discover the true cause I shall argue as follows: If an effect, which has succeeded with others at another time, does not take place with us, it necessarily follows that in our experiment there is something lacking which was the cause of the success of the former attempt; and, if we lack but one thing, that one thing is alone the true cause; now, we have no

---

[2] Francis Bacon called this the method of 'Degrees or Comparison'; cf. below, p. 292.

lack of eggs, nor of slings, nor of stout fellows to whirl them, and yet they will not cook, and indeed, if they be hot they will cool the more quickly; and, since nothing is wanting to us save to be Babylonians, it follows that the fact of being Babylonians and not the attrition of the air is the cause of the eggs becoming hard-boiled, which is what I wish to prove.

In its business of discovering proximate causes, Galileo held, science began with observations and observations had the last word. In accordance with the logic of science of the later Middle Ages, the method of 'resolution and composition,' he showed how to arrive at general theories by analysis from experience, to vary the conditions and isolate causes (as in the previous quotation), and to verify or falsify theories by experiment. Distinguishing the method used by Aristotle for investigation from that used in presenting his conclusions, Galileo said in the First Day of the *Two Principal Systems*:

I think it certain that he first obtained by the senses, by experiments and observations, such assurance as was possible of the conclusions, and that afterwards he looked for means to demonstrate them. For this is the normal course in the demonstrative sciences; and it is followed because, when the conclusion is true, by making use of the resolutive method one may hit upon some proposition already demonstrated or arrive at some principle known *per se*; but if the conclusion is false, one could go on for ever without ever finding any known truth—if indeed one does not encounter some impossibility or manifest absurdity. And you need have no doubt that Pythagoras, long before he had found the proof for which he offered the hecatomb, was sure that the square on the side opposite the right angle in a right-angled triangle was equal to the squares on the other two sides. The certainty of the conclusion assists not a little to the discovery of the proof, meaning always in the demonstrative sciences. But whatever was Aristotle's method of procedure, whether the reasoning *a priori* came before the sense perception *a posteriori* or the other way round,

it is enough that Aristotle, as he said many times, preferred the experience of the senses to any argument.

He went on, in the Second Day: 'I know very well that one single experiment or conclusive proof to the contrary would be enough to batter to the ground . . . a great many probable arguments.'[3]

Clearly in this presentation of the role of experiment Galileo's scientific method resembles that of the scholastic philosophers of Oxford and Padua who had interpreted Aristotle in terms of Plato's dialectic and had applied the *reductio ad absurdum* to empirical situations (see above pp. 8, 17 *et seq.*). In his use of 'thought experiments' —but not of impossible imaginary experiments—Galileo also carried on established practices. But he made one advance of the greatest importance. He insisted, at least in principle, on making systematic, accurate measurements, so that the regularities in phenomena could be discovered quantitatively and expressed in mathematics

The significance of this advance is made very plain in his own comments on William Gilbert's work on magnetism (cf. below, p. 189 *et seq.*) in the Third Day of the *Two Principal Systems*. 'I am going to explain, with a certain likeness to my own,' he said, 'his method of procedure in philosophising, in order that I may stimulate you to read it. I know that you understand quite well how much a knowledge of events contributes to an investigation of the substance and essence of things; therefore I wish you to take care to inform yourself thoroughly about many events and properties that are found uniquely in the lodestone, and not in other stones or other bodies.' He continued: 'I have the highest praise, admiration, and envy for this author, who framed such a stupendous concept concerning an object which innumerable men of splendid intellect had handled without paying any attention to it . . . But what I might have wished for in Gilbert would be a little more of the mathematician, and especially a thorough grounding in geometry, a discipline which would

---

[3] Galileo seems to have thought that science advanced through a series of alternatives each decided by a crucial experiment.

have rendered him less rash about accepting as rigorous proofs those reasons which he put forward as *veræ causæ* for the correct conclusions he himself had observed. His reasons, candidly speaking, are not rigorous, and lack that force which must unquestionably be present in those adduced as necessary and eternal scientific conclusions.'

It was by his insistence on measurement and mathematics that Galileo combined his strictly experimental method with the second main characteristic of his new approach to science. This was to try to express the observed regularities in terms of a mathematical abstraction, of concepts of which no exemplaries need actually be observed but from which the observations could be deduced. From its consequences the hypothetical abstraction could then be tested quantitatively. Galileo's method of abstraction was explicitly an adaptation of the postulational method of Archimedes and Euclid. It was of revolutionary importance both for his own work and consequently for the whole history of science. Under the influence of the same Greek tradition, such abstractions had certainly been used in some medieval scientific investigations, for example the 'ideal balance' with weightless arms, the mathematical expressions postulated in dealing with problems of motion, and the geometrical devices postulated to 'save the appearances' in astronomy. Following the precedent of Democritus and Plato, the mathematicisation of 'form' and 'substance' found especially in 13th-century optics is another aspect of the postulational method of abstraction that Galileo was to exploit. But because of the strength of Aristotelian influence, most pre-Galilean science was in practice constricted by the dominance of naïve and direct generalisations from common-sense experience. Galileo's use of the method of mathematical abstraction enabled him firmly to establish the technique of investigating a phenomenon by specially arranged experiments, in which irrelevant conditions were excluded so that the phenomenon could be studied in its simplest quantitative relations with other phenomena. Only after these relations had been established and expressed in a mathematical formula did he re-intro-

duce the excluded factors, or carry his theory into regions not readily amenable to experimentation.

In Galileo's eyes, one of the principal assets of the Copernican system was that Copernicus had escaped from the naïve empiricism of Aristotle and Ptolemy and taken a more sophisticated attitude to theories used to 'save the appearances.' 'Nor can I sufficiently admire the eminence of those men's intelligence,' says Salviati, during the Third Day of the *Two Principal Systems*,

> who have received and held it [the Copernican system] to be true, and with the sprightliness of their judgements have done such violence to their own senses, that they have been able to prefer that which their reason dictated to them to that which sensible experiences represented most manifestly to the contrary . . . I cannot find any bounds for my admiration how reason was able, in *Aristarchus* and *Copernicus*, to commit such a rape upon their senses as in spite of them, to make herself mistress of their belief.

Galileo believed the mathematical theories from which he deduced the observations to represent the enduring reality, the substance, underlying phenomena. Nature was mathematical. This view he owed partly to the Platonism which had been popular in Italy, particularly in Florence, since the 15th century. One essential element of this Pythagorean Platonism, which had been made increasingly plausible by the success of the mathematical method in 16th-century physics, was the idea that the behaviour of things was entirely the product of their geometrical structure. During the Second Day of the *Two Principal Systems*, Salviati replies to Simplicio's assertion that he agreed with Aristotle's judgment that Plato had doted too much upon geometry. 'After all,' says Simplicio, 'these mathematical subtleties do very well in the abstract, but they do not work out when applied to sensible and physical matter.' Salviati points out that the conclusions of mathematics are exactly the same in the abstract and in the concrete. 'It would indeed be novel if the computations and ratios made in abstract numbers did not afterwards correspond to the

gold and silver coins and the merchandises in concrete. Do
you know what does happen, Simplicio? Just as the com-
puter who wants his calculations to deal with sugar, silk,
and wool must discount the boxes, bales, and other pack-
ings, so the mathematical scientist (*filosofo geometra*),
when he wants to recognise in the concrete the effects
which he has proved in the abstract, must deduct the ma-
terial hindrances, and if he is able to do so, I assure you
that things are in no less agreement than the arithmetical
computations.'

The faith that inspired nearly all science until the end
of the 17th century was that it discovered a real intelligible
structure in objective nature, an *ens reale* and not merely
an *ens rationis*. Kepler believed himself to be discovering
a mathematical order which provided the intelligible struc-
ture of the real world; Galileo said, during the First Day
of the *Two Principal Systems*, that of mathematical propo-
sitions human understanding was 'as absolutely certain . . .
as Nature herself.' In fact, though Galileo rejected the kind
of 'essential natures' the Aristotelians had been seeking,
he simply brought in another kind by the back door. He
asserted that since mathematical physics could not deal
with the non-mathematical, what was not mathematical
was subjective (see above, p. 86 *et seq*.; cf. below, p. 300
et seq.). As he affirmed in *Il Saggiatore*, question 6:

> Philosophy is written in that vast book which stands
> forever open before our eyes. I mean the universe; but
> it cannot be read until we have learnt the language and
> become familiar with the characters in which it is writ-
> ten. It is written in mathematical language, and the let-
> ters are triangles, circles and other geometrical figures,
> without which means it is humanly impossible to com-
> prehend a single word.

It was precisely in his attitude to these mathematical
'primary qualities' that Galileo the Platonist differed from
Plato himself. Plato had held that the physical world was
a copy or likeness of a transcendent ideal world of mathe-
matical forms; it was an inexact copy and so physics was
not absolute truth but, as he put it in the *Timaeus*, 'a

likely story.' Galileo by contrast asserted that the real physical world *actually consisted* of the mathematical entities and their laws, and that these laws were discoverable in detail with absolute certainty. In the transitional state of contemporary scientific thought his analysis of scientific method had two main purposes. On the one hand he wanted to show that the Aristotelian explanations were not explanations at all, were in fact answers to the wrong questions and totally inadequate to the problems being considered. By eliminating Aristotle's particular conception of the real essences of the physical world, with their various irreducible natural qualities, natural positions in the universe and natural motions, he would eliminate the whole Aristotelian opposition to the new mathematical physics and dynamics and to Copernicus. On the other hand, he wanted to show how to find the true solutions the true explanations revealing the actual essence and structure of the physical world, and to show how to give reasons for asserting that such explanations were certainly true. Both aims were necessary for his programme of reframing the questions to be asked in order to construct a true and universal mathematical science of motion.

Galileo's Platonism was thus of the same kind as that which had led to Archimedes being known in the 16th century as the 'Platonic philosopher,' and with Galileo mathematical abstractions got their validity as statements about nature by being solutions of particular physical problems. By using this method of abstracting from immediate and direct experience, and by correlating observed events by means of mathematical relations which could not themselves be observed, he was led to experiments of which he could not have thought in terms of the old common-sense empiricism.

His approach to the search for the mathematical laws of phenomena, for example of the acceleration of heavy bodies, the swinging of a pendulum, the trajectory of a cannon ball, or the motions of the planets, was in the traditional 'Euclidean' manner of searching for premisses from which to deduce the data of the phenomena. Setting up his theories on the Euclidean pattern, his whole procedure

was what he called an '*argomento ex suppositione.*' Galileo was a scientist who was most conscious of problems of method and philosophy. There are many references to the subject in both his main works, the *Two Principal Systems* and the *Mathematical Discourses and Demonstrations concerning Two New Sciences* (1638). He described his method fully also in a letter to Pierre Carcavy in 1637. Since it was impossible to deal at once with all the observed properties of a phenomenon, he first reduced it intuitively to its essentials. After this 'resolution' of the essential mathematical relations involved in a given effect, he set up a 'hypothetical assumption' from which he deduced the consequences that must follow. This second stage he called 'composition.' Finally came an experimental analysis, which he also called 'resolution,' of examples of the effect in order to test the hypothesis by comparing its deduced consequences with observation. Abstraction was essential to the whole procedure. Thus, for example, in order to deal with a moving body dynamically it became a quantity of matter concentrated at its centre of gravity traversing a given space in a given time. It was strictly the 'physical object' so abstracted and defined that entered into the dynamical theorems. All questions relating to the 'nature' of the object in the Aristotelian sense were to be ignored. Thus Galileo was able to give precise formulation to a conception of motion first hinted at by Ockham and Buridan; and the methodological significance of his distinction between primary and secondary qualities becomes apparent in his treatment of motion kinematically in terms of velocity.

A good example of Galileo's method is his work on the pendulum. By abstracting from the inessentials of the situation, 'the opposition of the air, and line, or other accidents,' he was able to demonstrate the law of the pendulum, that the period of oscillation is independent of the arc of swing and simply proportional to the square-root of the length. This having been proved, he could then reintroduce the previously excluded factors. He showed, for instance, that the reason why a real pendulum, of which the thread was not weightless, came to rest, was not simply

because of air resistance, but because each particle of the thread acted as a small pendulum. Since they were at different distances from the point of suspension, they had different frequencies and therefore inhibited each other.

Another good example of his method is his study of freely-falling bodies, one of the foundations of 17th-century mechanics. Disregarding Aristotle's conception of motion as a process requiring a continuous cause, and the Aristotelian categories of movement based on purely 'physical' principles still accepted by such writers as Cardano or Kepler, he looked for a definition that would enable him to measure motion. He said, during the First Day of the *Two Principal Systems·*

> Let us call velocities equal, when the spaces passed have the same proportion as the times in which they are passed.

In this he followed such 14th-century physicists as Heytesbury and Richard Swineshead, whose works had been printed at the end of the 15th century and taught to Galileo during his youth at Pisa. He tried to arrange things so that he could study the problem under simple and controlled experimental conditions, for example in balls rolling down an inclined plane. He made a few preliminary observations, and analyzed the mathematical relations obtaining between two factors only, space and time, excluding all the others. Then he tried to invent what he called a 'hypothetical assumption,' which was a mathematical hypothesis from which he could deduce consequences that could be tested experimentally; and since, as Salviati said during the Second Day of the *Two Principal Systems,* 'Nature . . . does not do that by many things, which may be done by few,' he adopted the simplest possible hypothesis. During the Third Day of the *Two New Sciences,* 'On Local Motion,' he gave the definition of uniformly accelerated motion as a motion which, 'when starting from rest, acquires during equal time-intervals equal increments of velocity.' This, he said, he adopted for one reason, because Nature employs 'only those means which are most common, simple and easy.' His experimental verification consisted of a series

of measurements showing the concomitant variations in space travelled and time passed. If the consequences of his hypothesis were verified, he regarded this hypothesis as a true account of the natural order. If they were not, he tried again, until he reached a hypothesis which was verified; and then the particular instance, for example the observed facts about falling bodies, was explained by being shown to be the consequences of a general law. The object of science for Galileo was to explain the particular facts of observation by showing them to be consequences of such general laws, and to build up a whole system of such laws in which the more particular were consequences of the more general. In all this the role of intuition, even of an Aristotelian type although turned to a different purpose, was paramount. Intellectual intuition, abstraction, and mathematical analysis discovered the hypothetical possibilities; experiment became necessary to eliminate false hypotheses among these and to identify and verify the true one. An hypothesis so verified was a true intuitive insight into the details of the real structure of the physical world.

Galileo's approach to physical problems is clearly seen in the *Two New Sciences*, in his deduction of the kinematical laws of freely falling bodies, when Salviati turns away from the suggestion that certain physical causes might account for the facts and concentrates on the kinematical aspect of the problem.

> The present does not seem to be the proper time to investigate the cause of the acceleration of natural motion concerning which various opinions have been expressed by various philosophers, some explaining it by attraction to the centre, others by repulsion between the very small parts of the body, while still others attribute it to a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now, all these fantasies, and others too, ought to be examined; but it is not really worth while. At present it is the purpose of our Author merely to investigate and to demonstrate some of the properties of accelerated motion (whatever the cause of

this acceleration may be)—meaning thereby a motion, such that the momentum of its velocity goes on increasing after departure from rest in simple proportionality to time, which is the same as saying that in equal time-intervals the body receives equal increments of velocity;
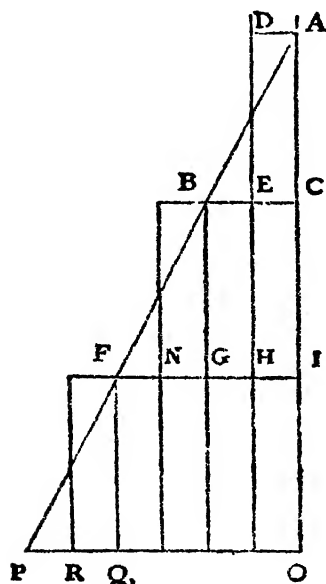
FIG. 4 *Diagram used in Galileo's proof that with a body falling with uniform acceleration, in successive equal intervals of time AC, CI, IO, the distances traversed (measured by the areas ABC, CBFI, IFPO) increase as 1, 3, 5, and so on. In modern terminology, assuming $v = at$, Galileo proved that $s = \frac{1}{2}at^2$.* From Discorsi e dimostratione matematiche intorno à due nuove scienze, *Bologna, 1655* (1st ed. Leiden, 1638), *Third Day.*

and if we find that the properties [of accelerated motion] which will be demonstrated later are realised in freely falling and accelerated bodies, we may conclude that the assumed definition includes such a motion of heavy

bodies and that their speed goes on increasing as the time and the duration of the motion.

This passage, indicating a classical turning-point in the history of science, was written in 1638, but Galileo had not always seen so clearly that the acceleration of free fall must be defined and the definition verified as a fact, before there could be any attempt at a dynamical explanation. Galileo's clarification of this distinction measures the progress he made between his early treatment of motion as a young professor at Pisa and his maturer understanding at Padua, to which he went in 1592. It opened the way to his attack on the dynamics itself and to his formulation, incomplete but definite, of the concept of inertial motion. This was the achievement of his period at Florence, to which he returned in 1610 under the special patronage of the Grand Duke of Tuscany.

Earlier discussions of free fall had never separated the kinematic from the dynamical aspects of the problem. The former were always presented as deductions from the latter and thus shared in their inadequacies, a characteristic found even in Soto's correct statement of the kinematic law (see above, p. 96 *et seq.*). No one had thought of simply ascertaining the correct kinematic law independently of any dynamics. In his first original scientific essays, the treatise and dialogue both entitled *De Motu* written at Pisa about 1590, Galileo followed this traditional procedure. The primary aim of these early essays was to refute the dynamical theory and law of motion on which Aristotle had based his arguments against the possibility of motion in a void, the basic assumption being that local motion was the resultant of a proportion between power and resistance to which both were necessary (see above, p. 48 *et seq.*). Galileo made criticisms of Aristotle's dynamics, and especially of his explanations of projectile motion and free fall, similar to those made by Buridan and Albert of Saxony and their followers, but the explanations he offered in their place suggest an attachment rather to the dynamics of Avempace than of Buridan, and to the Pythagorean or Platonic conception of relative gravity. He

asserted that a constant motive power would produce a finite uniform velocity through extended space even without any resistance, as for example in a void; if there were a resistant medium it would simply reduce this finite velocity by a definite amount. Projectile motion would thus be possible in a void; he explained it by the theory of *virtus impressa*. As to free fall, he said that every species of body had a finite natural velocity of fall determined by its intrinsic 'nature' or specific gravity, a velocity that would be realised in a void, where there was no resistance. In a resistant medium this natural velocity would be reduced by a definite amount determined by the relative specific gravities of the body and of the medium; indeed if the latter were the greater the body would rise. This left the problem of why heavy bodies accelerated when they fell from rest. To explain this Galileo supposed that in the case both of a body thrown upwards, and of one at rest above its natural place, a lingering upwards-directed *virtus* was acquired by the displacement from the centre; as the body fell, this *virtus* was gradually reduced, so that the body accelerated downwards until the opposing *virtus* had disappeared entirely, after which the body continued to fall with a constant velocity proper to its gravity. Thus Galileo did not at that time agree with his predecessors like Oresme who held that the acceleration of free fall would continue indefinitely, but rather had independently hit upon a theory proposed in antiquity by Hipparchus.

The physical-causal treatment of motion in these Pisan essays shows Galileo still very far from the kinematical approach for lack of the necessary concept of inertia. While criticising Aristotle, along somewhat traditional lines, he fully accepted the basic assumptions that a constant velocity required a constant motive power, and that an accelerated velocity required a corresponding increase in effective power. Another example of the same characteristics can be seen in his account in the essays of experiments of dropping different weights from 'a high tower.' These were later associated by Galileo's disciple and biographer Vincenzio Viviani with the Leaning Tower of Pisa, but there is no positive evidence that he actually made any

experiments from the Leaning Tower, and his manner of introducing them suggests rather that they were 'thought experiments' Thus in attacking Aristotle's assumption that speed of fall is proportional to weight, he speaks not only of flinging two stones, one twice as big as the other, from a high tower, but also of dropping two lead spheres, one a hundred times as big as the other, from the moon. He ridicules the notion that one stone will fall twice as fast as the other, and one sphere of lead 100 times as fast as the other. In fact Galileo's basic argument to demonstrate that bodies of the same material but different size would fall with the same speed was exactly that used by Benedetti: the whole cannot fall faster than the part (see above, pp. 132–33). But this did not apply to bodies, such as a piece of lead and a piece of wood, of different material. These fell with velocities proper to their 'natures' and, he wrote in the treatise *De Motu*, 'if they are let go from a high tower, the lead precedes the wood by a long space; and I have often made test of this. . . . Oh how readily are true demonstrations drawn from true principles!' he exclaimed.

Two other Italian scientists, Giorgio Coresio in 1612 and Vincenzio Renieri in 1641, did actually make such experiments from the Leaning Tower, and they found that even with bodies of the same material the heavier weight reached the ground first, if they were dropped from a sufficient height. Coresio even asserted that the velocity was proportional to the weight, thus confirming Aristotle's 'law'; but Renieri, giving actual figures, showed otherwise. In fact he submitted his results to Galileo, who referred him to his *Dialogue*. Discussing the subject more fully in his *Two New Sciences*, Galileo had pointed out that the actual difference in the velocity in such experiments was widely different from that to be expected from the Aristotelian 'law'. He was also aware that the results disagreed with the expectations of his new dynamics: by that time, having given up the conception of 'natures' as causes of motion, he had come to assume that all bodies of any material would fall with the same velocity. Unimpressed by the disagreement of experiment with theory, Galileo

made an abstraction from empirical actuality and said that the theory applied to free fall in a vacuum. In a resistant medium such as air, he said that a lighter body would be retarded more than a heavier one. Same results, different explanations! It has long ago ceased to be possible to regard the Leaning Tower experiment, even supposing Galileo made it, as in any sense crucial, or even new.

The first evidence that Galileo had successfully turned to a kinematical attack on the problem of free fall comes in his famous letter to Paolo Sarpi in 1604, in which he said that he had proved that the spaces passed over by a falling body were to each other as the squares of the times. By this time he must have assumed that the acceleration continued indefinitely, or would do so if it were not for the resistance of the air, which, as he explained in the *Two New Sciences*, tended to limit the velocity of a falling body to a maximum value. He claimed to have deduced his theorem, familiar now as $s = \frac{1}{2}at^2$, from the axiom that the instantaneous velocity was proportional to the *distance* fallen. He used in his demonstration the medieval geometrical method of dealing with varying qualities, taking the integral, Oresme's 'quantity of velocity' (the area ABC in Fig. 2), to represent the distance fallen (Fig. 4). But in fact, as Duhem showed, the axiom, or definition of uniform acceleration, that Galileo, by a curious error, actually assumed in his reasoning was not this impossible one already rejected by Soto, but that instantaneous velocity was proportional to *time*. Indeed the distinction between the two was not one that either the incompletely clear kinematics or mathematics of the period made easy. Exactly the same mistake was made by Isaac Beeckman and Descartes.

It seems likely that Galileo had discovered his mistake and correctly formulated the law of acceleration and the space theorem by 1609, although he published them only in the *Two Principal Systems* in 1632. It is possible that he had already carried out his experiment to test the law with a bronze ball rolling down an inclined plane as early as 1604. This experiment is described in the *Two New Sciences* (1638), where the mathematical demonstration is again set out. In the absence of an accurate clock, he

defined equal intervals of time as those during which equal
weights of water issued from a small hole in a bucket; he
used a very large amount of water relative to the amount
issuing through the hole, so that the decrease in head
was unimportant. His experiment confirmed his defini-
tion and law of free fall, and from it he deduced further
theorems.

It was this famous experiment that, on the empirical
side, distinguished Galileo's account from all preceding at-
tempts to deal with the problem of free fall, although it
is an indication of the contemporary lack of system in pre-
senting scientific results that Galileo recorded no actual
individual measurements, and gave only the conclusions he
had drawn from them. In fact Mersenne failed to get the
same results when he repeated Galileo's experiment some
years later—an indication perhaps of Galileo's confidence
in the mathematical and conceptual intuition to which he
owed his scientific success quite as much as to his experi-
ments. And it was just because he came to see the law of
acceleration and the space theorem within the theoretical
structure generated by the new concept of inertial motion
that they became the foundations of classical dynamics,
and may be considered, as Galileo himself believed them
to be, his greatest achievement.

Although the conception of motion developed in his
treatise *De Motu* was fundamentally anti-inertial, there
are to be found in it applications of the 'Platonic' technique
of abstraction in which the conception of inertia is already
foreshadowed. For example, in his discussions of a sphere
rolling on an infinite horizontal plane, a motion that is
neither natural nor violent and therefore can be produced
by an infinitely small force, or of the constant finite veloc-
ity of a body falling in a void—both cases being abstrac-
tions from empirical reality—he abolished by implication
the need for a continuing motive power to maintain con-
stant velocity. Later at Padua, just as had happened in the
14th century, he was to give up the theory of *virtus impressa*
as an explanation of projectile motion and natural accelera-
tion, in favour of a new theory of *impeto* or *momento*.
But Galileo's *impeto* belongs to another conceptual world

from Buridan's *impetus*. In Galileo's new dynamics *impetus* as a motive power became redundant: the imprecise idea of the conservation of motion that it contained became analysed into recognisable statements of the laws of inertia (still incompletely generalised by Galileo) and of the conservation of momentum.

In the Second Day of the *Two Principal Systems*, Galileo makes Salviati ask 'whether there is not in the movable, besides the natural inclination towards the opposite direction, another intrinsic and natural quality (*qualità*) which makes it resistant to motion. So tell me once more: Do you not believe that the tendency of heavy bodies to move downwards, for example, is equal to their resistance to being driven upwards?' To which Sagredo replies: 'I believe it to be exactly so, and it is for this reason that two equal weights in a balance are seen to remain steady and in equilibrium, the heaviness of one weight resisting being raised by the heaviness with which the other, pressing down, seeks to raise it.'

This passage contains in unanalysed form the distinction to be made by Isaac Newton (1642–1727) between weight, the force moving a falling body, and mass, the intrinsic resistance to motion.[4] It was in fact implied by Galileo's supposition that in a vacuum all bodies would fall with the same acceleration, differences in weight being exactly counterbalanced by equal differences in mass (cf. Vol. I, p. 115, note). It was impossible for Galileo to make this distinction clearly, because for him weight was still an in-

---

[4] Arising out of the problem of condensation and rarefaction as discussed by Aristotle, the principle was established in the 14th century that the *quantitas materiæ* of a body remained constant in all changes. The term *quantitas materiæ* was used by Giles of Rome. Following the work of Roger Swineshead (who also called it *massa elementaris*), Heytesbury and Dumbleton, Richard Swineshead developed a clear concept of the mathematical measurability of *quantitas materiæ* by the ratio of density and volume. With Buridan it became a dynamical concept (see above, p. 68, note). But weight (*pondus*) remained for the scholastics a property only of 'heavy' bodies, and so they were never able to conceive weight as proportional to mass as Newton did. I am again indebted to Dr. Weisheipl for some of this information: cf. above, p. 89, note.

trinsic tendency downwards, not something depending on an *external* relationship with another attracting body, such as had been suggested by Gilbert and Kepler on the analogy of magnetism (see below, p. 199 *et seq.*) and was to be generalised by Newton as the theory of universal gravitation. Nevertheless the conception that there was an intrinsic resistance (*resistanza interna*) to motion, equal to the weight or quantity of matter of a body, gave Galileo his definition and measure of *momento*, and enabled him to take up the problem of the persistence of motion in a manner that made the concept of inertia inevitable.

From the observation that on a balance a large weight placed a short distance from the fulcrum would oscillate in

FIG. 5 *Galileo's demonstration of inertia with the pendulum. From* Discorsi e dimostrazione matematiche intorno à due nuove scienze, *Bologna, 1655 (1st ed. Leyden, 1638),* Third Day.

equilibrium with a small weight placed a proportionately longer distance from the fulcrum, he derived the idea that what persists in motion is the product of weight multiplied by velocity. This product he called *impeto* or *momento*, not a cause of motion like Buridan's *impetus*, but an effect and measure of it. The problem of the persistence of motion was thus the problem of the persistence of *momento*, or momentum. He assumed, in the *Two New Sciences*, Third Day, that the momentum of a given body falling down a frictionless inclined plane was proportional only

to the vertical distance, and independent of the inclination; from this he concluded that a body falling down one plane would acquire momentum that would carry it up another to the same height. The swinging bob of a pendulum was equivalent to such a body, and he showed that if released at C (Fig. 5) it would ascend to the same horizontal line DC, whether it went by the arc BD or, when the string was caught by the nails E or F, by the steeper arcs BG or BI. This result he developed as follows:

> Furthermore we may remark that any velocity once imparted to a moving body will be rigidly maintained as long as the external causes of acceleration or retardation are removed, a condition which is found only on horizontal planes; for in the case of planes which slope downwards there is already present a cause of acceleration, while on planes sloping upwards there is retardation; from this it follows that motion along a horizontal plane is perpetual; for, if the velocity be uniform, it cannot be diminished or slackened, much less destroyed. Further, although any velocity which a body may have acquired through natural fall is permanently maintained so far as its own nature is concerned, yet it must be remembered that if, after descent along a plane inclined downwards, the body is deflected to a plane inclined upward, there is already existing in this latter plane a cause of retardation; for in any plane this same body is subject to a natural acceleration downwards. Accordingly we have here the superposition of two different states, namely, the velocity acquired during the preceding fall which, if acting alone, would carry the body at a uniform rate to infinity, and the velocity which results from a natural acceleration downwards common to all bodies.

As he had already argued in the *Two Principal Systems*, perpetual motion was the limiting case, reached in an ideal world without friction, as the acceleration and retardation given respectively by downwards and upwards sloping planes each gradually tended to zero with the approach of the planes to the horizontal. The *impeto*, or momentum, impressed on a body by its movement then persisted in-

definitely. Thus motion was no longer conceived of as a process requiring a cause commensurate with the effect but, as Ockham had foreshadowed, was simply a state of the moving body persisting unchanged unless acted on by a force. Force could therefore be defined as that which produced, not velocity, but a *change* of velocity from a state either of rest or of uniform motion. Further, when a body was acted on by two forces, each was independent of the other. Galileo assumed for practical purposes that the uniform motion preserved in the absence of an external force would be rectilinear, and this enabled him to calculate theoretically the trajectory of a projectile. In the *Two New Sciences*, Third Day, he showed that the path of a projectile, which moved with a constant horizontal velocity received from the gun and a constant acceleration vertically downwards, was a parabola, and that the range on a horizontal plane was greatest when the angle of elevation was 45 degrees. There could be no better proof than this theorem of the superiority of the theoretician able to foresee yet unobserved results, over the pure empiricist who could see only the facts already observed. As he said:

> The knowledge of a single fact acquired through the discovery of its causes prepares the mind to ascertain and understand other facts without need of recourse to experiments, precisely as in the present case, where by argument alone the Author proves with certainty that the maximum range occurs when the elevation is 45°. He thus demonstrates what perhaps has never been observed in experience, namely, that of other shots those which exceed or fall short of 45° by equal amounts have equal ranges.

Even more emphatic was Salviati's assertion in the Second Day of the *Two Principal Systems:* 'I am certain, without observation, that the effect will happen as I tell you, because it must so happen.'

Galileo certainly arrived by implication at the conception of inertial motion, which was the illumination of mind that made it possible for Newton to complete the terres-

trial and celestial mechanics of the 17th century; but Galileo himself did not state the law of inertia completely. He was investigating the geometrical properties of bodies in the real world, and in the real world it was an empirical observation that bodies fell downwards towards the centre of the earth. Thus, adapting the Pythagorean theory, he regarded gravity as the natural tendency of bodies to proceed to the centre of the collection of matter in which they found themselves, and weight as an innate physical property possessed by bodies; this was the source of movement or *impeto*. Galileo remained faithful all his life to the basic assumption, already found in the dialogue *De Motu*, that gravity was the essential and universal physical property of all material bodies. Confining his physical investigations to terrestrial bodies, he could take the centre of the earth to determine favoured directions in space, even though space itself was empty, homogeneous extension. The only 'natural' properties he left to bodies were their weight and their equivalent inertial 'internal resistance' to change in a motion. 'Natural gravity' was the only force he considered. It was thus in a form taking account of these assumptions that he expressed his version of the law of inertia As he wrote in the Third Day of the *Two New Sciences*:

> Just as a heavy body or system of bodies cannot move itself upwards, or recede from the common centre towards which all heavy things tend, so it is impossible for any heavy body of its own accord to assume any motion other than one which carries it nearer to the aforesaid common centre. Hence, along the horizontal by which we understand a surface, every point of which is equidistant from this same common centre, the body will have no momentum (*impeto*) whatever.

In the real world, therefore, the 'plane' along which movement would continue indefinitely was a spherical surface with its centre at the centre of the earth. As he said in the *Two Principal Systems*, Second Day:

A surface which is neither declining nor ascending ought in all its parts to be equally distant from the centre. . . . Then a ship moving over a calm sea is one of those movables which run along a surface that is neither declining nor ascending, and, if all external and accidental obstacles were removed, it would thus be disposed to move incessantly and uniformly from an impulse once received? I conclude, he said in the First Day, that only circular motion can naturally suit bodies which are integral parts of the universe as constituted in the best order, and that the most that can be said for rectilinear motion is that it is assigned by nature to the bodies and their parts only where these are disposed outside their natural places, in a bad order, and therefore in need of being restored to their natural state by the shortest path. From which it seems to me that it may reasonably be concluded that for the maintenance of perfect order among the parts of the universe, it is necessary to say that movable bodies are movable only circularly; and if there are any that do not move circularly, these are necessarily immovable, there being nothing but rest and circular motion apt to the conservation of order.

This conception of motion enabled Galileo to say that the circular motion of the heavenly bodies, once acquired, would be retained. Moreover, he said that it was impossible to prove whether the space of the real universe was finite or infinite. His universe thus contained bodies with independent physical properties, which affected their movements in real space. The same line of thought can be seen in the remark in the *Two Principal Systems* that a cannon ball without weight would continue horizontally in a straight line, but that in the real world, where bodies had weight, the movement which bodies conserved was in a circle. For practical purposes of calculation he assumed, as in his work on the trajectory of a projectile, that it was rectilinear motion that was conserved. But his conception of motion enabled him to say that in the heavenly bodies circular motion would be conserved. He did not have to explain their movements by gravitational attraction.

The intellectual revolution which had cost the 'Tuscan artist' such an anguish of effort, and yet left him still just short of reducing physics completely to mathematics, made it possible for his followers to take the geometrisation of the real world as evident. Cavalieri got rid of gravity as an innate physical property, and said that like any other force it was due to external action. Evangelista Torricelli (1608–47) regarded gravity as a dimension of bodies similar to their geometrical dimensions. Giordano Bruno (1548–1600), continuing the scholastic discussion of plural worlds and the infinity of space, had realised that Copernicus, in making it plausible to take any point as the centre of the universe, had abolished absolute directions (see below, p. 168 et seq.). He had popularised the idea that space was actually infinite and therefore without favoured natural directions. The French philosopher and mathematician, Pierre Gassendi (1592–1655), whose predecessors in the 16th century had, unlike the Italians, sometimes tended to identify the continuous quantity of geometry with physical extension, identified the space of the real world with the abstract, homogeneous, infinite space of Euclidean geometry. He had learnt from Democritus and Epicurus to conceive of space as a void, and from Kepler to regard gravity as an external force (see below, p. 191 et seq.). He therefore concluded, in his *De Motu Impresso a Motore Translato* published in 1642, that since a body moving by itself in a void would be unaffected by gravity, and since such a space was indifferent to the bodies in it, as Aristotle's space and its remnants in Galileo were not, the body would continue in a straight line forever. Gassendi thus first published the explicit statement that the movement which a body tended to conserve indefinitely was rectilinear, and that a change in either velocity or direction required the operation of an external force. He also first consciously eliminated the notion of *impetus* as the cause of motion. With the complete geometrisation of physics, the principle of inertial motion thus became self-evident.

Gassendi had been anticipated in the expression of this principle, though not in its publication, by René Descartes (1596–1650) in his book *Le Monde*, begun some time

before 1633. But if Descartes can thus be claimed as the
first to have given expression to the complete principle of
inertia, one fundamental and in the end fatal distinction
between his and Galileo's methods of procedure must be
emphasised. Whereas Galileo reached his incomplete in-
ertial principle as a deduction from a principle of conserva-
tion of momentum supported by physical reasoning,
Descartes based his complete principle on an entirely meta-
physical assumption of God's power to conserve movement.
Descartes had intended *Le Monde* to be a system of celes-
tial mechanics based on the Copernican theory, but, dis-
couraged by the condemnation of Galileo in 1633 for the
similar excursion made in the *Two Principal Systems* (see
below, p. 199 *et seq.*), he dropped the project, and the
incompleted work was not published until 1664, when its
author was already dead. The mechanical ideas contained
in *Le Monde* he again resumed in the *Principia Philoso-
phiæ* (1644). Carrying to an extreme, which Galileo had
been unable to realise, the notion that the mathematical
was the only objective aspect of nature, he said that matter
must be understood simply as extension (see below, pp.
306–7). In creating the universe of infinite extension God
also gave it motion. All sciences were thus reduced to meas-
urement and mathematics,[5] and all change to local mo-
tion. Motion, being something real, could neither increase
nor decrease in total amount, but could only be transferred
from one body to another. The universe therefore contin-
ued to run as a machine, and each body persisted in a state
of motion in a straight line, the geometrically simplest form
in which God set it going, unless acted on by an external
force. Only a void was indifferent to the bodies in it and,

---

[5] 'In order to be able to prove by demonstration everything that
I will deduce, I do not accept any principles in physics that are not
also accepted in mathematics; these principles are sufficient, be-
cause all the phenomena of nature can be explained by means of
them,' *Principia Philosophiæ*, II, 64. When mathematics was
used to explain physical events the necessary requirement was
that 'all the things which are deduced should agree completely
with experience,' *Princ. Philos.*, III, 46. Descartes' position in
the Augustine-Platonist tradition was thus similar to that of Grosse-
teste or Roger Bacon.

since Descartes accepted the Aristotelian principle that extension, like other attributes, could exist only by inhering in some substance, he held that space could not be a void, which was nothing, but must be a *plenum*. Only a *tendency* to continuous velocity in a straight line would therefore be possible in the real world. For Descartes the real world was simply geometry realised; movement he conceived of simply as a geometrical translation, with time as a geometrical dimension like space. The great mistake that resulted from this treatment was that Descartes completely failed to understand how to measure quantity of motion and thus failed to grasp the essential concept of the conservation of momentum. The movement that was always in a straight line was motion at an instant, conceived purely kinematically without any non-geometrical properties of inertia.

This theory left Descartes with the problem of the curvilinear motion of the planets. Having rejected action at a distance and all causes of deflection from inertial motion except mechanical contact, he could not accept a theory of gravitational attraction. He therefore tried to explain the facts by vortices in the *plenum*. He considered the original extension to have consisted of blocks of matter, each of which revolved rapidly about its centre. The consequent attrition then produced three kinds of secondary matter, characterised by luminosity (sun and stars), transparency (inter-planetary space, i.e., ether), and opacity (earth). The particles of these matters were not atomic but divisible to infinity, and their geometrical shapes accounted for their various properties. They were all in contact, so that motion could occur only by each successively replacing the next and thus producing a vortex, in which motion was transmitted by mechanical pressure (Pl. V). Such vortices carried the heavenly bodies round. Mechanical pressure was also the means of propagation of such influences as light and magnetism. The *plenum*, or ether, which owed some of its characteristics to Gilbert and Kepler, was thus loaded with the physical properties, among them what was later called 'mass,' that could not be reduced to geometry.

The vortex theory shows Descartes empirically at his

weakest, and Newton was to demonstrate in the *Principia Mathematica* (1687) that it would not in fact yield Kepler's laws of planetary motion and so was falsified by observation (cf. below, pp. 197–98).

In spite of his great contributions to mathematics and to the mathematical techniques of physics, Descartes developed his cosmology to a considerable extent on entirely non-mathematical lines, and certainly it makes a striking contrast with Galileo's approach to physical problems. Starting from a background of scholastic physics, Galileo achieved his successes by eliminating the physical-causal elements from the problem of motion; his approach to dynamics was through kinematics, and although his passionate concern with the new astronomy gave him a general cosmological objective, his method was to try to solve each individual problem separately, to discover empirically what laws were in fact exhibited by the natural world, before facing the task of reassembling them into a whole. While appreciating Galileo's individual kinematic descriptions, Descartes found his work lacking in a total view of physics and his method of abstraction defective exactly at the point where Galileo had made it so effective: its turning away from the problem of physical causes. Commenting in 1638 on Galileo's recently published *Discourses concerning Two New Sciences*, Descartes characterised his own position by contrast, writing to Mersenne:

> I will begin this letter with my observations on Galileo's book. I find that in general he philosophises much better than the average, in that he abandons as completely as he can the errors of the schools, and attempts to examine physical matters by the methods of mathematics. In this I am in entire agreement with him, and I believe that there is absolutely no other way of discovering the truth. But it seems to me that he suffers greatly from continued digressions, and that he does not stop to explain all that is relevant to each point; which shows that he has not examined them in order, and that, without having considered the first causes of nature, he has merely sought reasons for certain particular effects;

and thus he has built without a foundation. A month later he wrote again: As to what Galileo has written about the balance and the lever, he explains very well what happens (*quod ita fit*), but not why it happens (*cur ita fit*), as I have done in my *Principles*.

Descartes was not alone in not accepting Galileo's methods as covering the complete range of physical problems; many physicists especially in France, for example Fermat, Mersenne and Roberval, shared his hesitations. It was just because Descartes himself took the opposite course of inquiring beyond the mathematical descriptions into physical causes and the nature of things, and of boldly constructing an entire system of science ranging from psychology and physiology through chemistry to physics and astronomy, writing a new *Timæus*, that his ideas became in many ways by far the greatest single influence on the history of science in the 17th century. They established the general line of thought even of those who, like Newton, were most critical of the Cartesian system in detail. Descartes approached physics as a philosopher. It must not be supposed that for that reason he did not appreciate the function of experiments or make them himself; certainly he did (cf. below, pp. 239 *et seq.*, 254 *et seq.*). But it was through his philosophical method and the universality claimed for its most fundamental results that he came to dominate the scientific thought of the period and to provide in one bold sweep at least something comprehensive and consistent to disagree with. Descartes saw the object of his philosophical method as the search by rational analysis for the simplest elements making up the world, 'simple natures' that could not be reduced to anything simpler and so had no logical definitions (see below, p. 305 *et seq.*). So far as the physical world was concerned he found these in extension and motion. 'If I am not deceived,' he wrote in *Le Monde*, 'not only these four qualities [heat, cold, wetness, dryness], but also all the others, and even all the forms of inanimate bodies, can be explained, without having to suppose anything else in their matter but motion, size, shape, and the arrangement of their parts.' From these 'simple natures,'

and from purely metaphysical principles, partly relating to the perfection and goodness of God, he then proceeded to deduce the laws that the actual world must follow. He admitted that his conclusions might be wrong in detail, and he gave up the attempt to reduce the complicated observed world, with its many unknown variables, to mathematical laws; hence the largely qualitative character of *Le Monde* and the *Principia Philosophiæ*. But of the correctness of his general aims and general conclusions he never had any doubts.

It was the most fundamental general conclusion of Descartes' mechanistic philosophy that all natural phenomena could eventually, when sufficiently analysed, be reduced to a single kind of change, local motion; and that conclusion became the most influential belief of 17th-century science. This, and the consequent doctrines of universal corpuscularity and the universality of action by physical contact, provided the 17th century with a new conception of nature in place of Aristotle's qualitative 'forms' or 'natures'; they provided scientists with a 'regulative belief' determining the form given to physical and physiological theories. The Cartesian philosophy of nature was the immediate subject of most of the controversies in which Newton and Newtonianism became involved; the *Principia Mathematica* (1687) itself, while pursuing the same very general aims as the *Principia Philosophiæ*, was written partly as a polemic against the details of the Cartesian system and the methods of arriving at them. Moreover it was not only in the philosophy of science that Descartes' influence was felt. Christian Huygens (1629–95) owed his scientific awakening to Descartes and never entirely deserted his point of view; and in the conception of kinetic energy found obscurely in Leibniz's conception of *vis viva* and fully developed in the 19th century, Descartes could claim to have originated a substantial contribution to dynamics.

The history of Cartesianism begins only in the middle of the 17th century and belongs to this volume only in reminding us that the direction of thought culminating in Galileo's method of abstraction and descriptive analysis of motion was balanced by another less willing to see physics

alienated, even temporarily, from the search for the nature and causes of things. So far as the inertial principle was concerned, it was not Descartes but Galileo who provided the conception of motion on which Huygens, Newton and others were to build the classical mechanics of the 17th century. The dynamical inquiries of these mathematicians, though leading to the enunciation of a number of separate principles whose connection with each other was not at the time always clearly understood, such as the law of falling bodies, the concepts of inertia, force and mass, the parallelogram of forces and the equivalence of work and energy, really involved only one fundamental discovery. This was the principle, established experimentally, that the behaviour of bodies towards one another was one in which accelerations were determined, the ratio of the opposite accelerations they produced being constant and depending only on a characteristic of the bodies themselves, which was called mass. It was a fact which could be known only by observation that two geometrically equivalent bodies would move differently when placed in identical relations with the same other bodies. Where Galileo had halted before the real world and Descartes, geometrising from abstract principles, hid this physical property in vortices, Newton made an exact mathematical reduction of mass from the facts of experience. The relative masses of two such bodies were measured by the ratio of their opposite accelerations. Force might then be defined as that which disturbed a body from a state of rest or uniform rectilinear motion, and the force between two bodies, for example that of gravitation, was the product of either mass multiplied by its own acceleration. Inertial motion was an ideal limit, the state of motion of a body acted on by no other. The problem that had been so puzzling to those who first questioned the Aristotelian law of motion, why, excluding the resistance of the medium, bodies of different masses fell to the earth with the same acceleration, then found its solution in the distinction between mass, a property of the body providing intrinsic resistance, and weight, caused by the external force of gravitation acting on the body. Differences in weight might be considered as exactly

counter-balanced by proportional differences in mass. And the same mass had a different weight according to its distance from the centre of the earth. When these conceptions were generalised by Newton, the old problems of the acceleration of freely falling bodies and of the continued motion of projectiles were finally solved; and when the same principles were carried once more into the sky in the theory of universal gravitation, Buridan's aspiration was realised, and the movements of the heavens, which Kepler had correctly described, were united with these homely phenomena in one mechanical system. This not only brought about the final destruction of the hierarchically-ordered finite world of irreducibly-different 'natures,' which had formed the Aristotelian cosmos; it was a vast illumination of mind. The principles, first effectively established by Galileo, on which the new mechanics were constructed then seemed finally justified by their success.

(2) ASTRONOMY AND THE
NEW MECHANICS

Though, after its arrival in Western Christendom in the 13th century, the Ptolemaic system had been commonly regarded as simply a geometrical calculating device, the need was felt for an astronomical system which would both 'save' the phenomena and also describe the 'actual' paths of the heavenly bodies through space. Since the 13th century, observation and the revision of tables had gone on in connection with the chronic desire to reform the calendar and with the practical demands of astrology and navigation. Regiomontanus had been summoned to Rome for consultation on the calendar in 1475, the year before he died, and his work was used by the Portuguese and Spanish ocean navigators. Some medieval writers, for instance Oresme and Nicholas of Cusa, had suggested alternatives to the geostatic system as a description of physical 'fact' and, in the early years of the 16th century, the Italian Celio Calcagnini (1479–1541) put forward in a vague form a theory based on the earth's rotation. His countryman,

Girolamo Fracastoro (1483–1553), attempted to revive the system of concentric spheres without epicycles. It was left for Nicholas Copernicus (1473–1543) to elaborate a system which could replace Ptolemy's as a calculating device and yet represent physical 'fact,' and also 'save' additional phenomena, such as the diameter of the moon, which according to Ptolemy's system should have undergone monthly variations of nearly a hundred per cent.

Copernicus was educated first at the University of Cracow and then at Bologna, where he studied law but also worked with the professor of astronomy, Domenico Maria Novara (1454–1504). Later he proceeded to Rome, to Padua where he studied medicine, and to Ferrara where he completed his law. The remainder of his life was spent at Frauenberg, a cathedral town in East Prussia, where he performed the functions of a cleric, doctor and diplomat and produced a scheme which was the basis of a reform of the currency. In the midst of this busy life he proceeded to reform astronomy. Here, though he made a few observations, his work was that of a mathematician. He is a supreme example of a man who revolutionised science by looking at the old facts in a new way. He took his data mainly from the *Epitome in Almagestum* (printed 1496) of Peurbach and Regiomontanus and from Gerard of Cremona's Latin translation of the *Almagest*, which was printed at Venice 1515. Novara, a leading Platonist, had taught him the desire to conceive of the constitution of the universe in terms of simple mathematical relationships. Inspired by this, he set about producing his new system.

Martianus Capella had preserved for the succeeding centuries Heraclides' theory that Mercury and Venus, whose orbits are peculiar in their restricted angular ranges from the sun (the other planets may be seen at any angular distance, or 'elongation,' from the sun), actually revolved round the sun, while the sun with the remaining heavenly bodies revolved round the earth. Heraclides was also reported to have let the earth revolve daily on its axis. Copernicus not only gave the earth a daily rotation but made the whole planetary system, including the earth, revolve round a static sun in its centre. His reluctance to publish this

theory, of which the manuscript was complete by 1532, seems to have depended largely on the fear that it would be considered absurd. He had been satirised on the stage near Frauenberg in 1531, and his anxiety would certainly have been confirmed had he lived to hear the comments of such diverse personalities as the Italian mathematician Francesco Maurolyco and the German revolutionary Martin Luther (1483-1546). 'The fool,' said Luther, 'would overturn the whole science of astronomy.' Copernicus eventually drew up a short summary (*Commentariolus*), which seems to have become known to the Pope, and in 1536 he was asked by Cardinal Nicolaus von Schönberg to make his theory known to the learned world. Georg Joachim (Rheticus), a professor at Wittenberg (who is notable for having introduced the improvement of making trigonometrical functions depend directly on the angle instead of on the arc), had journeyed to Frauenberg in 1539 to study Copernicus' manuscript, and in 1540 Rheticus published his *Narratio Prima de Libris Revolutionum* concerning it. Copernicus' work was thus well advertised when, having been seen through the press by Rheticus, it appeared at Nuremberg in 1543, dedicated to Pope Paul III under the title *De Revolutionibus Orbium Coelestium*. Its practical value was demonstrated when Erasmus Reinhold used it to calculate the *Prussian Tables* (1551), though these suffered from the inaccuracy of Copernicus' data, and when the figure for the length of the year given in *De Revolutionibus* was proposed, though not used, as the basis of the reform of the calendar instituted by Pope Gregory XIII in 1582. In spite of the cautious preface by Andreas Osiander, stating the contrary, Copernicus certainly considered the revolution of the earth as a physical fact and not a mere mathematical convenience. *De Revolutionibus* thus posed the problems that occupied the greater part of physics down to Newton.

The Copernican revolution was no more than to assign the daily motion of the heavenly bodies to the rotation of the earth on its axis and their annual motion to the earth's revolution about the sun, and to work out, by the old de-

vices of eccentrics and epicycles, the astronomical conse-
quences of these postulates (Fig. 6, Pl. VI).

It was in postulating the annual motion of the earth
that Copernicus made his great strategic advance in theory
over the medieval discussions of a reformed astronomy, and
opened the way for the full mathematical development of
a new system. For example, though Oresme had made the
earth spin on its axis, his system remained geocentric. There
were certain peculiarities in the mathematics of the geocen-
tric system that Copernicus may have noticed: the con-
stants of epicycle and deferent were reversed between the
lower planets (Mercury and Venus) and the upper ones;
and the sun's period of revolution appeared in the calcula-
tions for each of the five planets (see Fig. 6). Of the
steps by which he arrived at the conception of a heliocen-
tric system Copernicus has left no detailed account. He
described simply in the preface to *De Revolutionibus* how
he was urged to think out a new way of calculating the
motions of the spheres because he found that the mathe-
maticians disagreed among themselves, and used different
devices: concentric spheres, eccentric spheres, epicycles. He
concluded that there must be some basic mistake.

> Then when I pondered over this uncertainty of tra-
> ditional mathematics in the ordering of the motions of
> the spheres of the orb, I was disappointed to find that
> no more reliable explanations of the mechanism of the
> universe, founded on our account by the best and most
> regular Artificer of all, was established by the philoso-
> phers who have so exquisitely investigated other details
> concerning the orb. For this reason I took up the task
> of re-reading the books of all the philosophers which I
> could procure, exploring whether any one had supposed
> the motion of the spheres of the world to be different
> from those adopted by the academic mathematicians.

In this way he came across Greek theories of the double
motion of the earth, on its axis and round the sun, and
these he developed, following the example of his predeces-
sors who had not scrupled to imagine whatever circles they
required to 'save the appearances.'
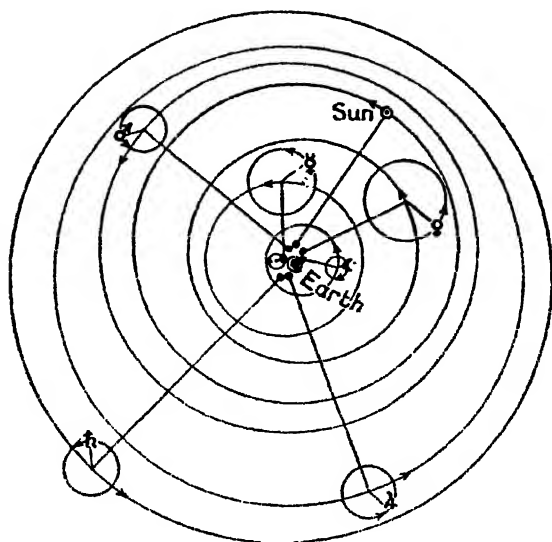
FIG. 6A, B *Comparison of the Ptolemaic* (A) *and Coperni-can* (B) *systems* (cf. *Vol. I, Figs. 2 and 3*). *Although his system was essentially a collection of independent devices for each heavenly body, the relative periods of revolution had established a traditional order of the orbits which Ptolemy adopted. By reversing the positions of the earth and the sun, Copernicus was able to use these periods to assess the relative mean distances of the planets from the sun and to rationalise the relationships between the epicycles and deferents of the lower (Mercury and Venus) and upper planets (see table below). The earth's motion round its orbit in the Copernican system is in fact repro-duced in the Ptolemaic system not only by the sun's orbit but also by the deferent of each lower planet (the planet's orbit being reproduced by Ptolemy's epicycle) and by the epicycle of each upper planet (the planet's orbit here be-ing reproduced by Ptolemy's deferent). It is not possible to show these points clearly in the diagram by drawing to scale. The positions of the centres of the planetary orbits relative to that of the sun's orbit in the Ptolemaic system, and to the sun itself in the Copernican system, are shown*
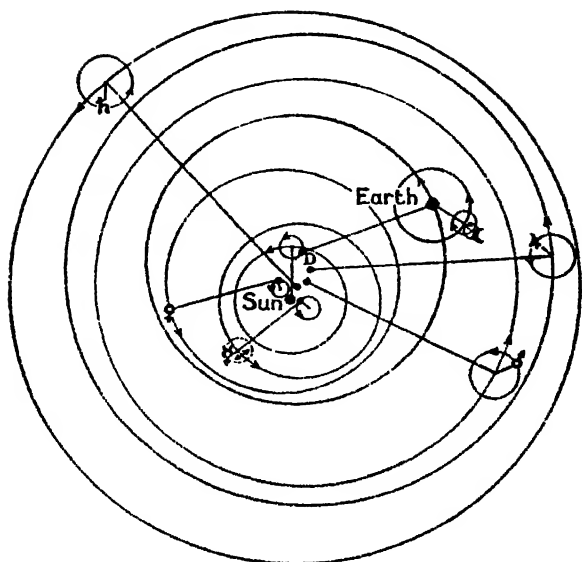
*by the dots at the inner ends of the radii of the deferents;
i.e., the large circles. Copernicus regarded his greatest technical achievement as the elimination of the objectionable
Ptolemaic equant (cf. Vol. I. p. 83), which he achieved
by referring the planetary motions not to the central sun
but to the centre (D) of the earth's orbit, which itself revolved round the sun on two further circles. This device
introduced inaccuracies in the planetary latitudes, especially of Mars, and it was Kepler who in fact made the sun
the point of reference for the planetary orbits (see Pl.
VII). Mercury received special treatment from Ptolemy,
who made the centre of its deferent rotate slowly round
another circle. Copernicus retained this device and in addition introduced the unique treatment of making the
planet oscillate, or "librate," on the diameter of its epicycle
instead of travelling round it. By a simple geometrical construction (not given here) it can be shown that any complexity introduced into one system in order to "save the appearances" can be matched in the other, so that the two
systems can be made equivalent in representing the angle*

'Occasioned by this,' he wrote, 'I also began to think of a motion of the earth, and although the idea seemed absurd, still, as others before me had been permitted to assume certain circles in order to explain the motions of the stars, I believed it would readily be permitted me to try whether, on the assumption of some motion of the earth, better explanations of the revolutions of the heavenly spheres might not be found. And thus, assuming the motions which in the following work I attribute to the earth, I have finally found, after long and careful investigation, that when the motions of the other planets are referred to the circulation of the earth and are computed for the revolution of each star, not only do the phenomena necessarily follow therefrom, but the order and magnitude of the stars and all their orbs and the heaven itself are so connected that in no part can anything be transposed without confusion to the rest and to the whole universe.

'Therefore,' he continued in book 1, chapter 10, 'we are not ashamed to maintain that all that is beneath the moon,

at which a planet appears when seen from the earth. But the two systems differ in their ranges of theoretical possibilities for the lower planets (Mercury and Venus), and these differences can provide an empirical test for choosing between them. According to the Copernican system, but not to the Ptolemaic, the lower planets can appear on the side of the sun remote from the earth (they cannot do so in the Ptolemaic system since they are inside the sun's orbit); their greatest angular distances from the sun are reached when earth-planet-sun form a right angle; and only they should show complete phases like the moon. Galileo confirmed these Copernican conclusions with his telescope (see pp. 184, 203 et seq.). The Ptolemaic system can, however, be made to yield the same conclusions by making the epicycles of Mercury and Venus rotate round the sun, a suggestion made by Heraclides of Pontus (see Vol. I, p. 88) and adopted by Tycho Brahe for the whole planetary system (see p. 179).

(a) Ptolemaic System

	Ratio of radii (corresponding to mean distance from sun in Copernican system)	Angular velocity (degrees per day)	Modern value of sidereal mean motion (degrees per day)
	Epicycle/Deferent	Epicycle	
Earth ⊕			
Moon ☽			
Mercury ☿	0.3708	4.09233	4.09234
Venus ♀	0.7194	1.62214	1.60213
Sun ☉		0.98563 (sun's orbit)	0.98561 (earth's orbit)
	Deferent/Epicycle	Deferent	
Mars ♂	1.5206	0.52406	0.52403
Jupiter ♃	5.2167	0.08312	0.08309
Saturn ♄	9.2336	0.03349	0.03346

(b) Copernican System

	Mean distance from sun expressed as ratio of distance of earth	Modern value	Period of revolution round sun (days)
Sun ☉			
		0.00257 (from earth)	27.33 (round earth)
Mercury ☿	0.3763	0.3871	88
Venus ♀	0.7193	0.7233	225
Earth ⊕	1.0000	1.0000	365.25
Mars ♂	1.5198	1.5237	687
Jupiter ♃	5.2192	5.2028	4.332
Saturn ♄	9.1743	9.5389	10.760

with the centre of the earth, describe among the other planets a great orbit round the sun which is the centre of the world; and that what appears to be a motion of the sun is in truth a motion of the earth; but that the size of the world is so great, that the distance of the earth from the sun, though appreciable in comparison to the orbits of the other planets, is as nothing when compared to the sphere of the fixed stars. And I hold it to be easier to concede this than to let the mind be distracted by an almost endless multitude of circles, which those are obliged to do who detain the earth in the centre of the world. The wisdom of nature is such that it produces nothing superfluous or useless but often produces many effects from one cause. If all this is difficult and almost incomprehensible or against the opinion of many people, we shall, please God, make it clearer than the sun, at least to those who know something of mathematics. The first principle therefore remains undisputed, that the size of the orbits is measured by the period of revolution, and the order of the spheres is then as follows, commencing with the uppermost. The first and highest sphere is that of the fixed stars, containing itself and everything and therefore immovable, being the place of the universe to which the motion and places of all other stars are referred. For while some think that it also changes somewhat [this refers to precession], we shall, when deducing the motion of the earth, assign another cause for this phenomenon. Next follows the first planet Saturn, which completes its circuit in thirty years, then Jupiter with a twelve years' period, then Mars, which moves round in two years. The fourth place in the order is that of the annual revolution, in which we have said that the earth is contained with the lunar orbit as an epicycle. In the fifth place Venus goes round in nine months, in the sixth Mercury with a period of eighty days. But in the midst of all stands the sun. For who could in this most beautiful temple place this lamp in another or better place than that from which it can at the same time illuminate the whole? Which some not unsuitably call the light of the world, others the soul or the ruler. Trismegistus calls it the visible God, the Electra of Sophocles the all-seeing.

So indeed the sun, sitting on the royal throne, steers the revolving family of stars.'

The consequences of Copernicus' postulates were of two kinds, physical and geometrical. The daily rotation of the earth encountered the Aristotelian and Ptolemaic physical objections, based on the theory of natural motions, concerning 'detached bodies,' an arrow or a stone sent into the air, and the strong east wind (see above, p. 78 *et seq.*). To these Copernicus replied in the same way as Oresme, making circular movement natural and saying that the air shared that of the earth because of their common nature and also perhaps because of friction. He held that falling and rising bodies had a double motion, a circular motion when in their natural place, and a rectilinear motion of displacement from, or return to, that place. The objection to this argument was that if bodies had a natural circular movement in one direction they should have a resistance, analogous to weight, to motion in the other. The answer to this, like that to the argument that the earth would be disrupted by what is now sometimes called 'centrifugal' force, which Copernicus merely said would be worse for the enormous celestial sphere if it rotated, had to await the mechanics of Galileo.

To the annual motion of the earth in an eccentric circle round the sun Copernicus' critics objected on three scientific grounds. First, it conflicted with the Aristotelian theory of natural movements, which depended on the centre of the earth being at the centre of the universe. To this Copernicus replied, with Oresme and Nicholas of Cusa, though abandoning Cusa's theory of balancing heavy and light elements, that gravity was a local phenomenon representing the tendency of the matter of any astronomical body to form spherical masses. The second objection arose from the absence of observable annual stellar parallaxes, or differences in position of the stars. Copernicus attributed this to the enormous distance of the stellar sphere from the earth compared with the dimensions of the earth's orbit. The third objection continued to be a stumbling-block till Galileo changed the whole conception of motion, when it ceased to be relevant. The Aristotelians maintained that

each elementary body had a single natural movement, but Copernicus gave the earth three motions: the two mentioned above which accounted, respectively, for the rising and setting of the heavenly bodies and for the passage of the sun along the ecliptic and the retrogradations and stations of the planets, and a third which was intended to account for the fact that the axis of the earth, notwithstanding the annual motion, always pointed to the same spot on the celestial sphere. This third motion was also made to account for the precession of the equinoxes and their illusory 'trepidations.'

With the sun and the celestial sphere, the boundary of the finite universe, at rest, Copernicus proceeded to provide the usual eccentrics, deferents and epicycles to account for the observed movements of the moon, sun and planets by means of perfect uniform circular motion. On the mathematical aspects of the result, Neugebauer in his *Exact Sciences in Antiquity* (1957, p. 204) comments as follows: 'The popular belief that Copernicus' heliocentric system constitutes a significant simplification of the Ptolemaic system is obviously wrong. The choice of the reference system has no effect whatever on the structure of the model, and the Copernican models themselves require about twice as many circles as the Ptolemaic models and are far less elegant and adaptable.' Copernicus' main mathematical contributions, according to Neugebauer, were three in number. He clarified the steps from observations to parameters, thus making a methodological improvement. He introduced with his system a criterion for assigning relative distances to the planets. And he suggested the proper solution of the problem of latitudes. But his belief in the imaginary trepidations of the equinoxes led to unnecessary complications and, by taking the centre of the earth's orbit as the centre of all the planets' motions, his treatment of Mars had considerable errors. Further, he relied on ancient and inaccurate data. This last defect was remedied by Tycho Brahe (1546–1601), who showed that the trepidations were due solely to errors in observation; and Johann Kepler (1571–1630), while considering Tycho's results, was to build his system from the orbit of Mars.

Copernicus had produced a mathematical system at least as accurate as Ptolemy's, with both mathematical advantages and disadvantages. Theoretically and qualitatively it was certainly simpler, in that he could give a unified explanation of a number of different features of planetary motion which in Ptolemy's system were arbitrary and disconnected. He could account for the retrogradations and stations of the planets as mere appearances due to a single movement of the earth, and could give a simple explanation of various motions peculiar to individual planets. In the 16th century it was also counted in his favour that he had reduced the number of circles required; he used 34. Copernicus had also argued that the postulated movements of the earth did not conflict with physics, that is, with Aristotle's physics. These arguments in favour of the heliostatic system were negative, and moreover in order to effect the reconciliation he had to interpret Aristotle's physics, just as Oresme had done, in a sense different from that accepted by most of his contemporaries. It is not surprising that many of them remained unconvinced. How then did Copernicus justify his innovation, both to himself and publicly, and why did it make so strong and so emotional an appeal later to Kepler and Galileo? A large part of the answer certainly lies in the Neoplatonism they all shared. In the passage already quoted from *De Revolutionibus*, book 1, chapter 10, Copernicus justifies the new system he sets out by an appeal to its simplicity (qualitative, not quantitative) and to the special position it gives to the sun. The intellectual biographies of Kepler and Galileo, and the manner in which they used these and similar arguments, show that they too had committed themselves to the heliocentric system because of their metaphysical beliefs, before they had found arguments to justify it physically.

The Copernican system appealed first to three types of interest. The *Alfonsine Tables* had caused dissatisfaction both because they were out of date and no longer corresponded to the observed positions of stars and planets, and because they differed from Ptolemy on the precession of the equinoxes and added other spheres beyond his 9th, de-

viations offensive to humanists who believed that the per-
fection of knowledge was to be found in the classical writ-
ings. All practical astronomers, whatever their views on the
hypothesis of the earth's rotation, thus turned to the 16th-
century *Prussian Tables* calculated on Copernicus' system,
though, in fact, these were scarcely more accurate. Some
humanists regarded Copernicus as the restorer of the
classical purity of Ptolemy. Another group of writers, such
as the physicist Benedetti, Bruno, and Pierre de la Ramée,
or, as he was called, Petrus Ramus (1515–72), saw in the
Copernican system a stick with which to beat Aristotle.
Finally, scientists like Tycho Brahe, William Gilbert
(1540–1603), Kepler and Galileo, came to face the full
meaning of *De Revolutionibus* and attempted to unify ob-
servations, geometrical descriptions and physical theory. It
was because of the absence of such a unity that until the
end of the 16th century, while everyone used the *Prussian
Tables*, no one advanced astronomical theory. Tycho Bra-
he's contribution was to realise that such an advance de-
manded careful observation, and to make that observation.

Tycho's main work was done at Uraniborg, the observa-
tory built for him in Denmark by the king. His first task
was to improve the instruments then in use. He greatly
increased their size, constructing a quadrant with a 19-foot
radius and a celestial globe 5 feet in diameter, and he im-
proved methods of sighting and graduation. He also de-
termined the errors in his instruments, gave the limits of
accuracy of his observations, and took account of the effect
of atmospheric refraction on the apparent positions of
heavenly bodies. It had been customary before Tycho to
make observations in a somewhat haphazard manner, so
that there had been no radical reform of the ancient data.
Tycho made regular and systematic observations of known
error, which revealed problems hitherto hidden in the pre-
vious inaccuracies.

Tycho's first problem arose when a new star appeared in
the constellation Cassiopeia on 11 November, 1572, and
remained until early in 1574. Scientific opinion received a
marked shock from this object. Tycho attempted to deter-
mine its parallax and showed that this was so small that the

star must be beyond the planets and adjacent to the Milky Way. Although he himself never fully accepted it, the mutability of celestial substance had thus been definitely demonstrated. Also, though comets had been regularly observed since the days of Regiomontanus, Tycho was able to show, with his superior instruments, that the comet of 1577 was beyond the sun and that its orbit must have passed through the solid celestial spheres, if these existed. He also departed from the Platonic ideal and suggested that the orbits of comets were not circular but oval. Further, Aristotelian theory held that comets were manifestations in the air. It is significant that, although it would have been possible with instruments available from antiquity to show that comets penetrated the unchanging world beyond the *moon*, such observations were not in fact made until the 16th century. In 1557 Jean Pena, royal mathematician at Paris, had maintained on optical reasoning that some comets were beyond the moon and hence had rejected the spheres of fire and of the planets. He held that air extended to the fixed stars. Tycho went further and abandoned both the Aristotelian theory of comets and the solid spheres. At the same time, the discovery of land scattered all over the globe led other natural philosophers, such as Cardano, to abandon the theory of concentric spheres of earth and water based on the Aristotelian doctrine of natural place and motion. Land and sea they held to form one single sphere.

While Tycho provided the observations on which to base an accurate geometrical description of heavenly motions, he was led by physical as well as by Biblical difficulties to reject the rotation of the earth. He did not consider that Copernicus had answered the Aristotelian physical objections. Further, before the invention of the telescope had revealed the fact that the fixed stars, unlike the planets, appear as mere luminous points and not as discs, it was usually held that they shone by reflected light, and their brightness was taken as a measure of their magnitude. Tycho therefore deduced, from the absence of observable annual stellar parallax, that the Copernican system would involve the conclusion that the stars had diameters of incredible dimensions. He produced a system of his own

(1588), in which the moon, sun and fixed stars revolved round a stationary earth while all the five planets revolved round the sun. This was geometrically equivalent to the Copernican system, but escaped what he considered to be the latter's physical defects and included the benefits of his own observations. It remained an alternative to Copernicus (or Ptolemy) during the first half of the 17th century, and when Tycho bequeathed his observations to Kepler, who had come to work with him, he asked him to use it in the interpretation of his data.

Kepler did more than this. Michael Mästlin (1550–1631), under whom he had first studied, had, like Tycho, also calculated the orbit of the comet of 1577, and he declared the Copernican system alone capable of accounting for it. Kepler persisted in this opinion. He was also strongly influenced by Pythagoreanism. The vision of abstract harmony, according to which he believed the world to be constructed, sustained him through the drudgery of arithmetical computation to which he was consigned both by his astronomical researches and by his work as a professional astrologer. Throughout his life he was inspired by the search for a simple mathematical law which would bind together the spatial distribution of the orbits and the motions of the members of the solar system. After numerous trials he arrived at the idea published in his *Mysterium Cosmographicum* (1596), that the spaces between the planetary orbits each corresponded, from Saturn to Mercury, to one of the five regular solids or 'Platonic bodies': cube, tetrahedron, dodecahedron, icosahedron and octahedron. His object was to show the necessity of there being six and only six planets and of their orbits being of the relative sizes they are, as calculated from their periods round the sun. He tried to show that the five regular solids could be fitted to the six orbits so that each orbit was inscribed in the same solid about which the next outer orbit was circumscribed. He then went to Tycho Brahe, who had moved to Prague, from whom alone he could get the correct values of the mean distances and eccentricities that would confirm this theory. He was forced instead to set it aside, but his mathematical vision came to perceive in

1. Nicole Oresme with an Armillary sphere. From *Le Livre du Ciel et du Monde*, Bibliothèque Nationale, Paris, Ms français 565 (XIV cent.).
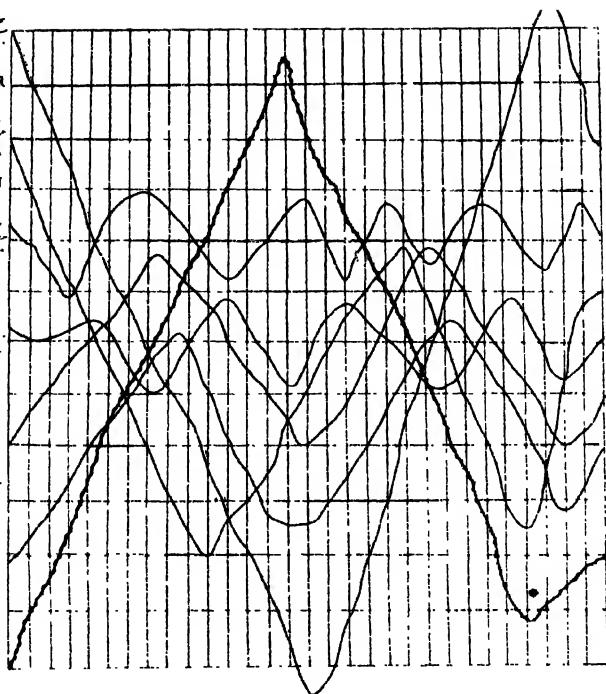
11. The earliest-known graph; showing the changes in latitude (vertical divisions) of the planets relative to longitude (horizontal divisions). From MS Munich 14436 (XI cent.).

Aprés cela prenant vn point a difcretion dans la courbe,
comme C, fur lequel ie fuppofe que l'inftrument qui fert
a la defcrire eft appliqué, ie tire de ce point C· la ligne
C B parallele a G A, & pourceque C B & B A font deux
quantités indeterminées & inconnuës , ie les nomme
l'vne y & l'autre x. mais affin de trouuer le rapport de
l'vne à l'autre, ie confidere auffy les quantités connuës
qui determinent la defcription de cete ligne courbe,
comme G A que ie nomme a, K L que ie nomme b, &
N L parallele a G A que ie nomme c. puis ie dis, comme
N L eft à L K, ou c à b, ainfi C B, ou y, eft à B K, qui eft
par confequent $\frac{b}{c}y$: & B L eft $\frac{b}{c}y - b$, & A L eft $x +$
$\frac{b}{c}y - b$. de plus comme C B eft à L B, ou y à $\frac{b}{c}y - b$, ainfi
a, ou G A, eft à L A, ou $x + \frac{b}{c}y - b$. de façon que mul-
<div align="center">S f</div> tipliant

III. A page from Descartes, La Géométrie (1637), in which
he discusses the algebraic equation of parabola.

IV. The mathematical disciplines and philosophy. The student is met by Euclid at the outer gate. Inside he finds Tartaglia surrounded by the mathematical disciplines: Arithmetic, Music, Geometry, Astronomy, Astrology, etc. A cannon is firing, showing the trajectory of the projectile. At the far gate stand Aristotle and Plato, to welcome the student into the presence of Philosophy. Plato holds a scroll with the inscription 'Let no one untrained in geometry enter here' (cf. p. 6, Vol. 1). From N. Tartaglia, *Nova Scientia*, Venice, 1537.
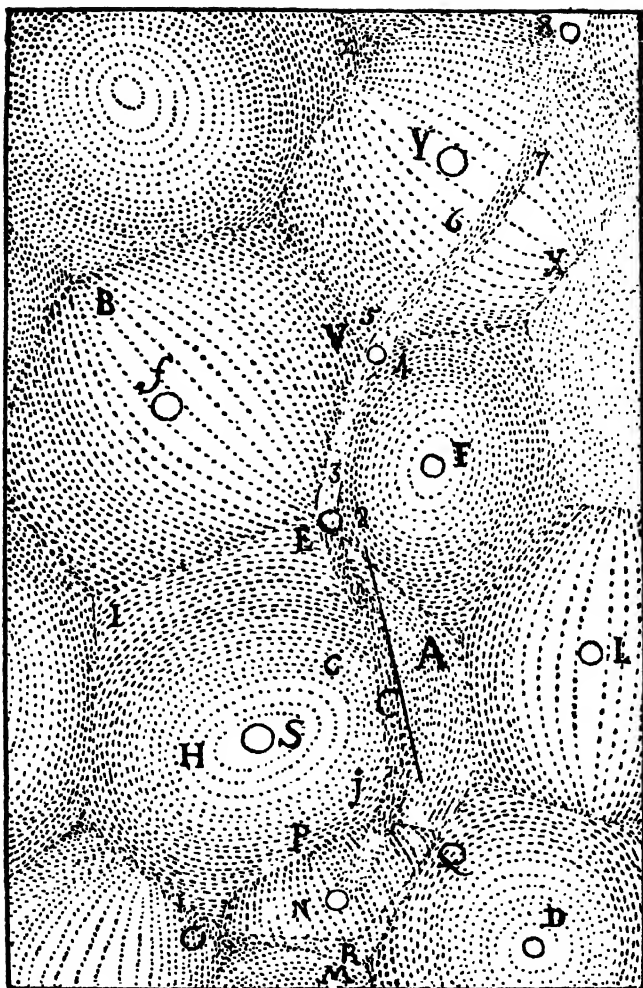
v. Diagram of vortices, from Descartes, *Principia Philoso-phiæ*, Amsterdam, 1644. Planets are carried in the whirlpool of subtle matter round the sun S. A comet, escaped from a vortex, is seen descending by an irregular path from the top right. Descartes thought that it would be impossible to reduce the motion of comets to law.
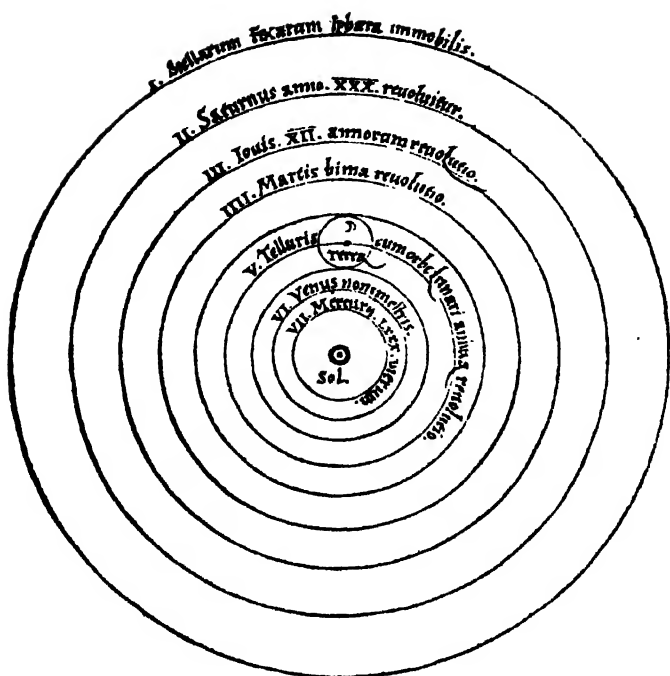
VI. The Copernican system. From Copernicus, *De Revolutionibus Orbium Cœlestium*, Nuremburg, 1543.
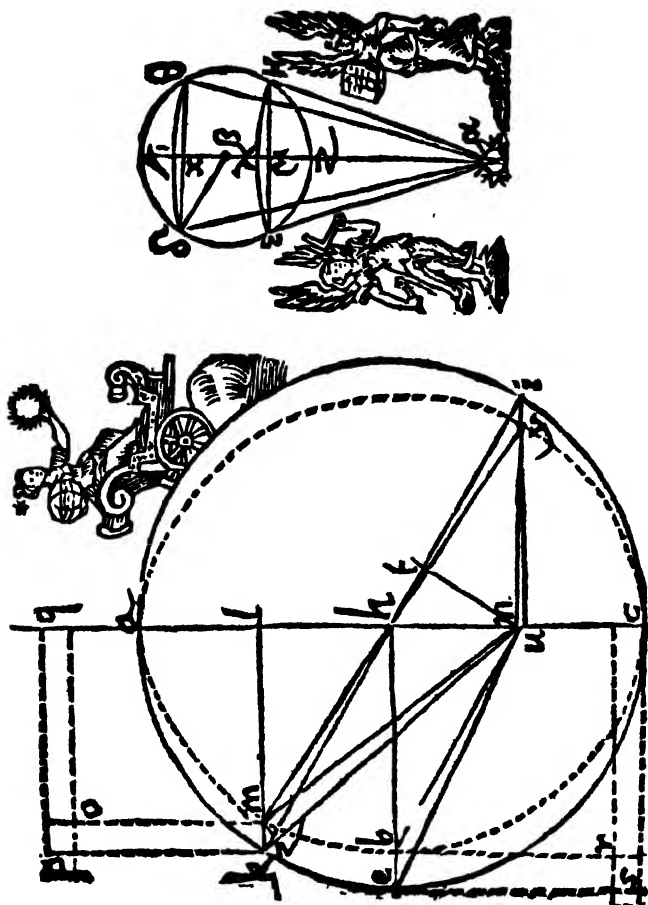
vii. Kepler's demonstration of the elliptical orbit of Mars
If the sun is at one focus (n) of the ellipse (the curve shown
by the broken line) and the planet at m, then according to
Kepler's second law the radius nm sweeps out equal areas
in equal times. The small diagram on the right is part of
Kepler's proof of the equivalence of motion on an ellipse to
that on a deferent and epicycle. From Astronomia Nova,
Prague, 1609.

1 610   My first observation of the new planet.

octob 17

1610

My first observation & others following of the new-found planets about Jupiter.

VIII. Page from Thomas Harriot's papers at Petworth House, describing his observations on Jupiter's satellites made at Syon House, on the Thames near Isleworth, and from the roof of a house in London. Harriot knew of Galileo's discovery of the satellites on 7 January 1610, but as early as July 1609 he had himself been observing the moon with a telescope (cf. pp. 184, 253). The upper part of the page is a rough entry of his first observations, and the lower part is the beginning of a fair copy he afterward wrote out. See note on p. 335.
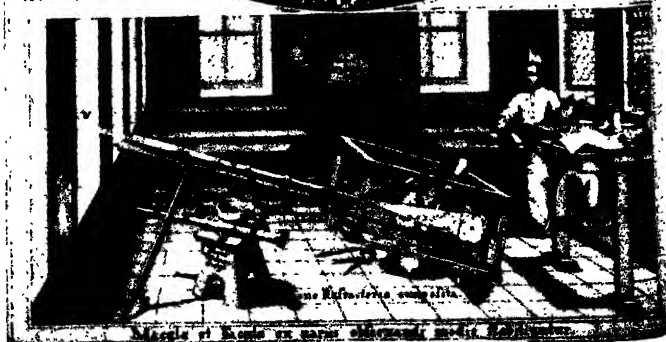
IX. Telescope and other instruments in use, and an apparatus
for showing sun spots by projection on to a screen. From C.
Scheiner, Rosa Ursina, Bracciani, 1630.
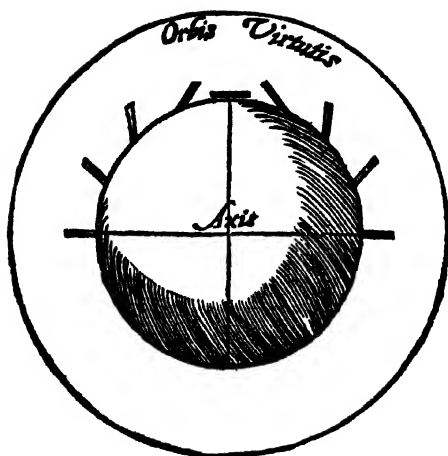
x. The earth as a magnet, and magnetic dip. From Gilbert,
De Magnete, London, 1600.
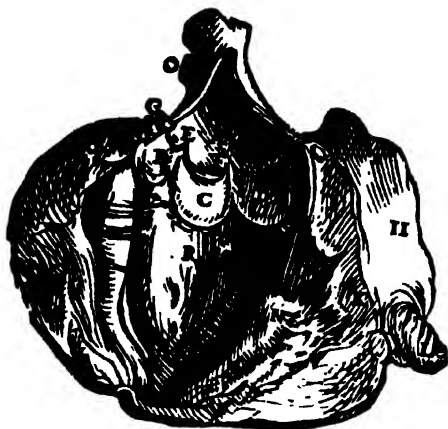
xi. The heart and its valves. From Vesalius, *De Humani
Corporis Fabrica*, Basel, 1543.

XII. Leonardo's drawing of the heart and associated blood vessels. From *Quaderni d'Anatomia 4*, Royal Library, Windsor, MS by Gracious permission of H.M. the Queen. See note on p. 335.
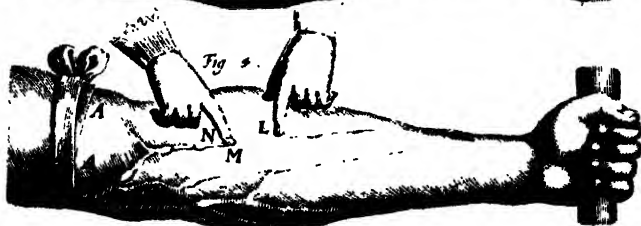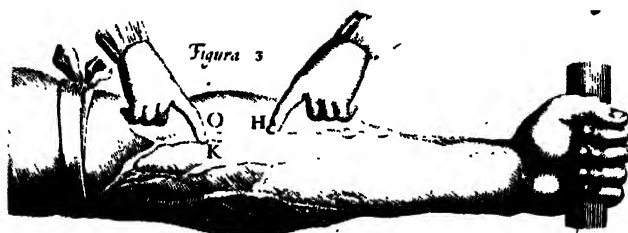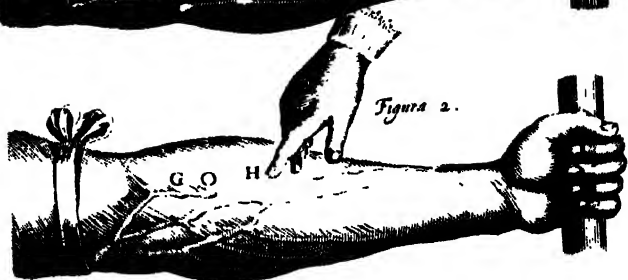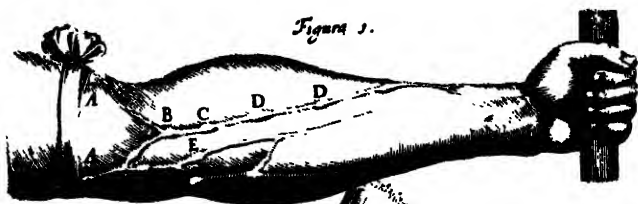
XIII. Harvey's experiments showing swelling of nodes in veins at the valves. From *De Motu Cordis*, London, 1639 (1st ed. 1628).

Tycho's data the foundations of celestial harmony. Having worked out the orbit of Mars on each of the three current theories, the Ptolemaic, the Copernican and the Tychonic, he saw that Copernicus had unnecessarily complicated matters by not allowing the planes of all the planetary orbits to pass through the sun. Even when this assumption was made there was still an error of 8 or 9 minutes in the arc of the orbit of Mars; and this could not be attributed to inaccuracy in the data. This forced him to abandon the assumptions that planetary orbits were circular and the movements of the planets uniform, and led him to formulate his first two laws: (1) Planets move in ellipses with the sun in one focus; (2) each planet moves, not uniformly, but so that a line joining its centre to that of the sun sweeps out equal areas in equal times (*Astronomia Nova aitiologetos, seu Physica Cœlestis tradita commentariis de motibus stellæ Martis ex observationibus G. V. Tychonis Brahe*, 1609– Pl. VII).

It was actually the second of these laws that Kepler discovered first. One of the difficulties encountered was the considerable variation in the velocity of Mars round its orbit, so that it was faster nearer the sun than remote from it. He first tried to render this variation mathematically by reintroducing the equant, which Copernicus had rejected. But he found that there was no equant that permitted the accurate calculation of all the observations. His proof that the same variations occurred in the earth's orbit demonstrated mathematically the similarity of its motion to those of the planets. He saw the problem then as that of finding a theorem relating the velocity of a planet's rotation at any point to its distance from the sun in an eccentric orbit. This he solved by a method of integration by which he showed that the duration of the planet on a very small arc of its trajectory was proportional to its distance from the sun. Guided in his approach to this problem by his physical conception of a power or *virtus* extending from the sun and moving the planets, it followed that this motive power was inversely proportional to the distance from the sun. Thus the motive power was inversely proportional to the duration of the planet on an arc of its

orbit—a conclusion entirely in keeping with the Aristotelian dynamical assumption that velocity requires a motive power.

It was in the course of these calculations and the checking of the predicted positions against Tycho Brahe's data that Kepler began to have his revolutionary doubts whether the planetary orbits were really circular. He had decided to give up the circular movements in 1604. As he wrote in his *Astronomia Nova*, part 3, chapter 40:

> My first error was to take the planet's path as a perfect circle, and this mistake robbed me of the more time, as it was taught on the authority of all philosophers, and is consistent in itself with metaphysics.

The fact that Kepler came to break what Koyré has called the 'spell of circularity,' while Galileo did not, makes an interesting contrast in the character of their Platonism. Galileo denied the Platonic ontological distinction between geometrical figures and material bodies; so far as he could he saw the physical world as geometry realised; and this made it difficult for him to deny the privileged status of circularity in physics and astronomy while accepting it in mathematics and, as has been shown recently, in æsthetics (cf. above, pp. 141–42, 158). Kepler, on the other hand, by retaining the ontological distinction between ideal form and material realisation, was able without violence to his Platonic metaphysics to accept a deviation from circularity forced on him by the empirical data. He argued that celestial bodies, *qua* bodies, were bound to deviate from the perfectly circular course because their motions were not the work of mind but of nature, of the 'natural and animal faculties' of the planets, which followed their own inclinations, as he said in his *Epitome Astronomiæ Copernicanæ*, book 4, part 3, chapter 1 (1620).

Again guided by his conception of the physical causes of planetary motion, Kepler at first supposed that the non-circular orbit was an ovoid resulting from two independent motions, one caused by the sun's *virtus* and the other by a uniform rotation of the planet on an imaginary epicycle produced by a *virtus* of its own. Kepler found himself un-

able to deal mathematically with the various ovoid curves he tried, so he decided to use as an approximation the ellipse, of which the geometry had been fully worked out by Apollonius. He discovered that the ellipse fitted his law of areas perfectly, an empirical conclusion for which he later tried to give a physical explanation by means of an oscillating motion or 'libration' of the planet on the diameter of its epicycle (cf. Fig. 6, Mercury).

After ten years further labour he arrived at his third law, published in 1619 in the *Harmonice Mundi*: (3) the squares of the periods of revolution (p_1, p_2) of any two planets are proportional to the cubes of their mean distances (d_1, d_2) from the sun (C), that is, $\dfrac{p_1^2}{p_2^2} = \dfrac{d_1^3}{d_2^3}$. This was a law for which Kepler had searched from the beginning of his career, but he made his discovery in the end almost accidentally. Following the method of trial and error, he made a series of comparisons of the instantaneous velocities and the periods and the distances of the different planets, but reached no significant formula. Eventually he tried comparisons of powers of these numbers, and found that those of his 'third law' gave an exact empirical fit.

These laws could scarcely have been formulated without the work of Greek geometers, especially Apollonius, on conic sections. This subject had been developed by Maurolyco and, in a commentary on Witelo (1604), by Kepler himself. In deducing his second law, Kepler made a contribution to mathematics, introducing the innovation, from which considerations of strict logic had restrained the Greeks, of considering an area as made up of an infinite number of lines generated by revolving a given curve about an axis (cf. above, p. 129). For the integration required for his second law he used a method similar to that by which Archimedes had determined the value of π. The work of the practical astronomer was also greatly assisted by improvements in methods of computation, first by the systematic use of decimal fractions introduced by Stevin, but above all by the publication in 1614 of the discovery of logarithms by John Napier (1550–1617). Following this, other mathematicians calculated tables for trigonometrical

functions and accommodated logarithms to the natural
base *e*. The slide rule was invented by William Oughtred
in 1622. Kepler made use of some of these innovations in
reducing to order the practical results of his own and Ty-
cho's work for the *Rudolphine Tables*, published in 1627.

Kepler's three laws provided a final solution of the an-
cient problem of discovering an astronomical system which
would both 'save' the phenomena and describe the 'actual'
paths of the bodies through space. Copernicus' 'third mo-
tion' of the earth was abandoned since, there being no
celestial spheres, the phenomena it was supposed to ex-
plain were attributed simply to the fact that the earth's
axis remained parallel to itself in all positions. The inde-
pendent invention of the telescope (with magnifications of
up to about thirty) by Galileo added confirmation for the
'Copernican' theory. One of Tycho's objections to this the-
ory was removed when Galileo was able to show, by finding
the distance at which a stretched cord of known thickness
would just eclipse them, that the fixed stars were not of the
incredibly enormous dimensions Tycho had supposed they
would have to be, on the assumption that brightness was
proportional to magnitude, in order to be as bright as they
are at a distance sufficient for them to show no parallax.
Galileo also resolved parts of the Milky Way into individ-
ual stars, and he confirmed Copernicus' deduction that
Venus, because of the position he held it to have inside
the earth's orbit, would have complete phases like the
moon. The other lower planet, Mercury, also has complete
phases, whereas Mars has only partial phases (cf. Fig. 6).
In 1631 Pierre Gassendi observed the transit, which Kepler
had predicted, of Mercury across the sun's disc, and estab-
lished that it described an orbit between the sun and the
earth. The transit of Venus was observed in 1639 by the
English astronomer Jeremiah Horrocks (1619–41). Gali-
leo, in his *Sidereus Nuncius* (1610), described the moun-
tains on the moon and the four satellites of Jupiter, which
he took as a model of Copernicus' solar system (cf. Pl.
VIII). Later he observed Saturn as misshapen (his tele-
scope would not resolve the rings), and he was able to show
that the variations in the apparent sizes of Mars and Venus

corresponded with the distances of these bodies from the earth according to the Copernican hypothesis. His observation of spots on the sun, by which he claimed to estimate its rate of rotation, also added evidence against the Aristotelian theory of immutability. Sun-spots were also described by Johann Faber and by the Jesuit Father Christopher Scheiner (1611), who soon afterwards constructed a telescope embodying improvements suggested by Kepler.

The astronomical theory of the early years of the 17th century was thus the achievement of the practical alternation of hypothesis and observation which had proceeded since Copernicus. Kepler gave an account of his conception of the philosophy and methods of astronomy in the first book of his text-book, *Epitome Astronomiæ Copernicanæ* (1618). He conceived astronomy to begin with observations, which were translated, by means of measuring instruments, into lengths and numbers for treatment by geometry, algebra and arithmetic. Next, hypotheses were formed which brought the observed relations together in geometrical systems which 'saved the appearances.' Finally, physics studied the causes of the phenomena related by an hypothesis, which must also be consistent with metaphysical principles. The whole inquiry sought to discover the true planetary motions and their causes. at present hidden in 'God's pandects' but to be revealed by science.

Kepler's achievement was in fact much more than simply to discover the true descriptive laws of planetary motion; he also made the first suggestions towards a new physical cosmology into which they would fit. That he did not succeed in this attempt is in part a measure of the extreme difficulty of the problem, which was solved only when Newton united Kepler's planetary laws with the completion of Galileo's terrestrial dynamics by means of the bridging law of universal gravitation. Towards that bridging law Kepler supplied both a positive contribution and a direction of inquiry. Following the preface to *De Revolutionibus*, the view had become common that, as Francis Bacon expressed it in his criticism of Copernicus in his *Novum Organum* (book 2, aphorism 36), the heliostatic system had been 'invented and assumed in order to abbre-

viate and ease the calculations,' but was not literally and physically true. 'There is no need for these hypotheses to be true, or even to be at all like the truth,' Osiander had written in this preface; 'rather, one thing is sufficient for them: that they should yield a calculus which agrees with the observations.' It was Kepler who first detected that Copernicus had not written these words. He disagreed with them strongly. The goal of the inquiry, he insisted, was to discover how the planets actually moved, and not only how but why they moved as they did and not otherwise: 'so that I might ascribe the motion of the Sun to the earth itself by physical, or rather metaphysical reasoning, as Copernicus did by mathematical,' as he said in the preface to the *Cosmographic Mystery.*

In fact Kepler made his discoveries of the three laws of planetary motion in a search for something much more, in the course of a metaphysical inquiry behind the visible appearances into the underlying harmonies expressed in purely numerical relations which he held to constitute the nature of things: the *harmonice mundi* which became manifest in the planetary motions and in music—an actual 'music of the spheres.' A reader unprepared for the individualities of Kepler's processes of thought may find the bulk of his difficult writings, concerned as much with questions like the nature of the Trinity, of celestial harmony, and of the relation of divine to human knowledge as with astronomy, an almost unintelligible matrix into which gems of science somehow got embedded. This would be to misunderstand the organisation of his thought entirely; and it would be to miss an obvious clue to perhaps the most important element in any original scientific thinking: the bridge of intuition and imagination by which he crossed the logical gap from the immediate results of observation to the theory by means of which he explained these results. All the evidence points to the bridge in Kepler's mind being made by the preconceptions of the metaphysical inquiries of which his science formed a part. Developed first in analogy to the relations between the persons of the Trinity, his conception of the structure of the universe became part of a theological creed. But it was also part of Kepler's

presuppositions—a point that came out vividly in a controversy on the subject with the English Rosicrucian Robert Fludd (cf. below, p. 249)—that the true structure and harmonies of the universe were those verified in observation. After his first visit to Tycho Brahe in 1600 he wrote in a letter to his friend Herwart von Hohenburg:

> I would have concluded my research on the harmonies of the world, if Tycho's astronomy had not fascinated me so much that I almost went out of my mind; still I wonder what could be done further in this direction. One of the most important reasons for my visit to Tycho was the desire, as you know, to learn from him more correct figures for the eccentricities in order to examine my *Mysterium* and the just mentioned *Harmonice* for comparison. For these speculations *a priori* must not conflict with experimental evidence; moreover they must be in accordance with it.

In developing this criterion of empirical confirmation he took account of the range of confirmation, asserting for example that the Copernican hypothesis was 'truer' than the Ptolemaic because, of the two, it alone could put the planets in an order round the sun according to their periods. Kepler's laws of planetary motion and his attempts to explain them were thus, so to speak, carved out of his preconceived Neoplatonic metaphysics by as strict an application as possible of quantitative methods and of the principle of the empirical test. It is this that makes him so interesting an example of scientific thinking, so different from that to be expected from a too literal application of the austerities of a positivist or 'operationalist' interpretation or of the canons of J. S. Mill.

Kepler's central metaphysical conception was of the existence from eternity in the mind of God of archetypal ideas, which were reproduced on the one hand in the visible universe and on the other in the human mind. Of these geometry was the archetype of the physical creation and was innate in the human mind. As he wrote in 1599 to Herwart von Hohenburg:

To God there are, in the whole material world, material laws, figures and relations of special excellency and of the most appropriate order. . . . Let us therefore not try to discover more of the heavenly and immaterial world than God has revealed to us. Those laws are within the grasp of the human mind; God wanted us to recognize them by creating us after his own image so that we could share in his own thoughts. For what is there in the human mind besides figures and magnitudes? It is only these which we can apprehend in the right way, and if piety allows us to say so, our understanding is in this respect of the same kind as the divine, at least as far as we are able to grasp something of it in our mortal life. Only fools fear that we make man godlike in doing so; for God's counsels are impenetrable, but not his material creation.

To this conception he joined the ancient doctrine of the *signatura rerum,* of the signs of things, according to which the external form of a thing was held to point to properties and a level of reality that were not directly visible. In the *Cosmographic Mystery* he described at length the visible universe as a sign or image of the Trinity, having the most perfect form of the sphere: the Father was represented by the centre, the Son by the outer surface, and the Holy Ghost by the radius having an equality of relationship between centre and surface.[6] In creating the visible universe in accordance with this geometrical symbolism, God placed at the centre a body to represent the Father by its radiation of power and light: this was the sun. Following the precedent of earlier Neoplatonic cosmologies, for example that of Grosseteste (see Vol. I, p. 74), Kepler conceived all natural powers as flowing out from bodies to assume a spherical form; and so by analogy with the power emanating from the Father, the sun became the instrument giving visible shape and life to the cosmos and everything in it, a universe in which everything was animate. It was the *anima motrix* or 'motive soul' of the sun that hurried the

[6] An analogous symbolism, differently arranged, is found in the medieval Celtic cross.

planets round in their circular orbits, and also the comets, with a velocity depending on its power after it had reached their respective distances. It has been suggested that it was because Kepler approached the problem of the planetary motions with this archetypal image in mind that he became a convinced Copernican.[7] Certainly he never abandoned the *animæ motrices* as the 'physical' motive power, even after he had been forced by the observational data he obtained from Tycho Brahe to give up the circular orbits. He was encouraged in his continued use of these causal conceptions as a guide to his mathematical inquiries by the explanations William Gilbert had given of his recent discoveries in magnetism.

Gilbert was court physician to Queen Elizabeth, who gave him a pension to pursue his research. He took a considerable interest in astronomy, but his main achievement was to work systematically through an entire field of scientific inquiry, the field of magnetism and electricity as then capable of study. Gilbert's *De Magnete* (1600), though containing some measurements, was entirely non-mathematical in treatment, and is the most striking illustration of the independence of the experimental and mathematical traditions in the 16th century (cf. above, p. 139). He derived his methods largely from Petrus Peregrinus, whose work had been printed in 1558, and from practical compass-makers such as Robert Norman, a retired mariner whose book, *The Newe Attractive* (1581), contains the independent discovery of the magnetic dip. This had been observed first by Georg Hartmann in 1544. Gilbert extended Peregrinus' work to show that the strength and range of a uniform lodestone was proportional to size. He also showed that the angle of dip of a freely suspended needle varied with latitude. Peregrinus had likened the needle-lines traced on a spherical magnet to meridia, and called the points where they met poles. Gilbert inferred from the orientations in which magnets set with respect to the earth that the latter was itself a huge magnet with its poles at the geographical poles. This he confirmed by

[7] See C. G. Jung and W. Pauli, *The Interpretation of Nature and the Psyche*, London.

showing that iron ore was magnetised in the direction in which it lay in the earth. The properties of lodestones and of the compass were thus included in a general principle (Pl. X).

Gilbert also made a study of electrified bodies, which he called *electrica*. He showed that not only amber, but also other substances such as glass, sulphur and some precious stones, attracted small things when rubbed; he identified a body as being 'electric' by using a light metallic needle balanced on a point. He pointed out that while the lodestone attracted only magnetisable substances, which it arranged in definite orientations, and was unaffected by immersion in water or screens of paper or linen, electrified bodies attracted everything and heaped them into shapeless masses and were affected by screens and immersion. Niccolo Cabeo (1585–1650) later observed that bodies flew off again after being attracted; Sir Thomas Browne said they were *repelled*.

Gilbert's empiricism extended only as far as the facts he had established. He used a balance to disprove the old story, accepted by Cardano, that the magnet fed on iron, but his explanations of magnetism and electricity, though not inconsistent with the facts, did not arise out of them. His explanation was really an adaptation of Averroës' theory of 'magnetic species' in a setting of Neoplatonic animism. Beginning with the principle that a body could not act where it was not, so that all action by means of matter must be by contact, he asserted that if there appeared to be action at a distance there must be a material 'effluvium' responsible for it. Such an effluvium, he held, was released from electrified bodies by the warmth of friction. He excluded magnetic action from this explanation, because, since it could pass through matter, it could not be due to a material effluvium; the motion of iron towards a magnet was more like that of a self-moving soul. But he extended the theory of effluvia to explain the earth's attraction for falling bodies, the effluvium here being the atmosphere. Without going into details, he attributed the diurnal rotation of the earth, which he accepted, to magnetic

energy, and the orderly movements of the sun and planets to the interaction of their effluvia.

Kepler was himself interested in magnetism, and Gilbert's work stimulated him to use this phenomenon to explain the physics of the universe. Here he accepted the current Aristotelian conception of motion as a process requiring the continuous operation of a motive power. As a young man under Scaliger he had adopted the Averroïst doctrine of Intelligences moving the heavenly bodies, but he had afterwards abandoned it because he wanted to consider only mechanical causes. He explained the continued daily rotation of the earth on its axis by the *impetus* which God impressed on it at the creation. But, like Nicholas of Cusa, he identified this *impetus* with the earth's soul (*anima*), thus reinstating the equivalent of an Intelligence. This *impetus*, he held, did not corrupt, for, with the Pythagorean theory of gravity which he accepted, circular motion could, without contradiction, be considered the natural motion of earth. To answer the traditional objections to the earth's daily rotation he developed Gilbert's suggestions. He considered lines, or elastic chains of force, which he held to be magnetic, to emanate radially from the earth's *anima motrix* and carry round the moon, clouds and all bodies thrown above the surface of the earth. Similar lines from the *animæ motrices* of Jupiter and Saturn carried round their satellites, and lines from the sun carried round the whole planetary system as the sun turned on its axis. It was this theory of a magnetic force, which diminished as distance increased so that the velocity of a planet in its orbit varied inversely as the distance from the sun, that led him to his second law. The rotation of the sun swinging its magnetic lines in a vortex would move the planets in a circle; the deviation from this to produce an elliptical orbit he tried to explain by the oscillations caused by the attraction and repulsion of their poles. Further, just as the motive force of the sun was magnetic, so there was an analogy between magnetism and gravitation. Gravitation was the tendency of cognate bodies to unite and, if it were not for the motive power carrying the moon and earth

round their orbits, they would rush together, meeting at an *intermediate* point. This last was an entirely new idea.

It was Kepler's idea that a satellite was kept on its orbit by *two* forces, one the mutual radial attraction with the central body and the other the motive power of the *anima motrix* impelling it laterally, that made his physical system the opening into the unification of terrestrial and celestial dynamics by Newton. The beginning of Kepler's achievement in this direction was his development of the Pythagorean conception of gravity. Oresme, Copernicus, Gilbert and Galileo had all rejected Aristotle's conception of gravity as a tendency to move towards a special place, the centre of the universe, and substituted for it gravity as the tendency of cognate bodies to unite; and the analogy with magnetism had been made by more than one medieval writer before being exploited again by Gilbert. Kepler considered this tendency to be caused by a real attraction (*virtus tractoria*) exerted externally by one body or another. His innovation was to make the attraction (in gravitation as in magnetism) *mutual*, and then to express it in a dynamical form. He wrote in the introduction to his *Astronomia Nova:*

> If two stones were placed close together in any place in the universe outside the sphere of the power (*virtus*) of a third cognate body, they would, like two magnetic bodies, come together at an intermediate point, each moving such a distance towards the other, as the mass (*moles*) of the other is in proportion to its own.

Postulating that the earth and the moon were cognate bodies, like two stones, he continued:

> If the moon and the earth were not retained, each in its orbit, by their animal and other equivalent forces, the earth would ascend towards the moon one fifty-fourth part of the distance between them, and the moon descend towards the earth about fifty-three parts; and they would join together; assuming, however, that the substance of each is of one and the same density.

That the attractive force of the moon did actually extend to the earth he concluded from the ebb and flow of the tides, which he supposed to be caused by the moon pulling the water of the seas towards itself: a theory which Grosseteste had foreshadowed, reminding us once more of the persistence of the complex of ideas that went with Neoplatonism (cf. Vol. I, p. 126). Kepler supposed it likely that a much stronger force extended from the earth to the moon, and beyond.

Kepler developed this conception of gravitation only in application to the earth and the moon; he did not suppose, for example, that the sun and the planets were cognate bodies attracting each other. Similarly he failed to grasp the cosmological significance of the inverse-square law which he formulated as a photometric law relating the intensity of light to the distance from its source, for example the sun. Again displaying both his consistently 'realist' philosophy of science, and the complex of Neoplatonic associations that clung to all developments of the 'cosmology of light' (cf. Vol. I, pp. 74, 99–100, 126), he described, in the introduction to his *Astronomia Nova*, the course of his inquiries into the motive forces swinging the planets round.

> I have begun by saying that in this work I shall treat astronomy not on the basis of fictional hypotheses (*hypotheses fictitiæ*), but on the basis of physical causes, and that for this purpose I have found it necessary to proceed by stages. The first stage was the demonstration that the eccentrics of the planets meet in the body of the sun. Next, by deductive reasoning, I proved that since, as Tycho Brahe showed, the solid orbs do not exist, it follows that the body of the sun is the source and the seat of the force which makes all the planets revolve round the sun. I showed likewise that the sun performs this in the following manner: while remaining in the same place, the sun nevertheless rotates as if on a tower and in fact emits through the breadth of the world an immaterial species (*species*) from its body, analogous to the immaterial species of its light.
>
> This species, because of the rotation of the solar body, revolves in the form of a very fast vortex, which extends

through the whole immensity of the universe and carries the planets with it, drawing them round in a circle with a vehemence (*raptus*) which is intenser or weaker according to whether the density of this *species*, in accordance with the law of its flow (*effluxus*), is greater or less.

The interaction of the individual motors of the planets with this common motor then produced the deviation from the circle. So far, so good. Kepler had raised for the first time the question of what moved the planets, since the spheres did not exist.

In his *Ad Vitellionem Paralipomena* (1604) Kepler had shown that if, as he held, light and other powers (*virtus, species*) expanded in a sphere from their source, then their strength would decrease as the area of the surface of the sphere, that is in proportion to the square of the radius. But in his *Epitome Astronomiæ Copernicanæ* (book 4, part 2, chapter 3; 1620) he specifically denied that this photometric law applied to the motive force of the sun, which he said decreased in *simple* proportion to distance. He tried to argue that the inverse-square law applied only to the sun's light. His argument was that whereas the sun's light expanded in a sphere, so that its intensity decreased according to the areal increase in the surface of the sphere, the sun's motive force extended only in the *plane* of each planetary orbit and decreased with the linear increase in the circumference. Certainly he was very far from applying it to *attraction* between the sun and the planets.

Kepler in fact resembles Galileo in supplying elements towards a unifying principle of cosmology whose need he saw clearly but of whose realisation he stopped short. Their omissions are curiously complementary and have a curious symmetry in preparation for the Newtonian synthesis. Neither Galileo nor Kepler had really grasped the dynamical problem presented by the planets. Galileo believed like Copernicus that the planetary revolutions were a 'natural' motion; that is, they required no external mover and could be accepted on grounds of order alone. Galileo was able to hold this because he ignored Kepler's demonstration of the elliptical orbits, which he certainly knew. Whether he did so for metaphysical or æsthetic reasons,

or simply as he said in 1614 because Kepler's writing was 'so obscure that apparently the author did not know what he was talking about,' the result was that he continued to regard the planets as revolving in circles (cf. above, p. 158). In any case he did not admit that the planets required any forces, either lateral or centripetal, to keep them in their orbits. Thus by ignoring Kepler's descriptive laws Galileo failed to see that the actual geometry of the heavens vitiated any spherical model, and so he missed the problem of how the planets were retained in their elliptical orbits.

Kepler's attempt to solve this problem, on the other hand, was vitiated by his failure to grasp the full meaning of the inertial principle which had been clearly though incompletely stated by Galileo in his second *Letter on the Sunspots* in 1612.[8] Continuing to suppose that continuous uniform velocity required a continuous motive power, Kepler saw this supplied by the *species motrix* or *virtus motoria* supposed to emanate from the sun; and since these swung the planets round laterally, he did not suppose that a centripetal force was required to keep them in their orbits instead of flying off tangentially. He failed to see the universal significance of the model he had himself supplied of the earth and the moon.

The uncertainty which Kepler himself seems to have felt in his inquiries into the vast problems which he had undertaken are shown by the changes he made, after each failure, in his approach to scientific explanation.[9] After discovering that the planetary theory proposed in the *Mysterium Cosmographicum* would not fit the facts, he turned from a conception of satisfactory explanation as one in which mathematical harmonies are discovered in the chaos of observations, to a mechanical conception of the universe as a regulative and heuristic guide to the investigations published in *Astronomia Nova*. The title of this work is itself

Galileo's *Letter* was written in 1612 and published in 1613. It was Kepler who introduced the word *inertia* into physics but he used it to mean an intrinsic resistance to motion and inclination to rest if in motion.

revealing: *The New Astronomy or Physics of the Heavens explored on the Basis of the Law of Causality and developed in Analyses of the Movements of Mars based on Observations by Tycho Brahe.* While preparing this work he wrote to Herwart von Hohenburg in 1605:

> I am much occupied with the investigation of the physical causes. My aim in this is to show that the celestial machine is to be likened not to a divine organism but rather to a clockwork . . . , insofar as nearly all the manifold movements are carried out by means of a single, quite simple magnetic force, as in the case of a clockwork all motions [are caused] by a simple weight. Moreover I show how this physical conception is to be presented through calculation and geometry.

In the end the physical theory of the *species motrix* emanating from the sun, put forward in *Astronomia Nova*, also proved an empirical failure, for it was observed that the apparent speed of the sun's rotation, then believed to be measured by that of the sun-spots, did not agree with that of the planets. For his next work Kepler allowed his conception of mathematical harmony to satisfy him as a criterion of satisfactory explanation, and in the *Harmonice Mundi* he announced his Third Law without any attempt to deduce it from mechanical principles. Two quite distinct meanings were involved in this conception of 'harmony.' According to the first, the Second Law, for example, was harmonious because it showed the areal velocity as constant; and it is worth noting that just as Ptolemy's constant angular velocity was more abstract and removed from immediate observation than Aristotle's directly observable constant linear velocity, so Kepler's areal velocity was a discovery of constancy or uniformity at a further stage of abstraction. The second meaning of Kepler's harmony applied to the 'fitness' or 'rightness' of the structure of the universe, for example the sun's 'rightful place' in the centre. The two meanings seem to have no logical connection but both performed heuristic and regulative functions in all Kepler's work.

Because they could see only sections of the total picture that was to emerge later, the attempts that both Kepler
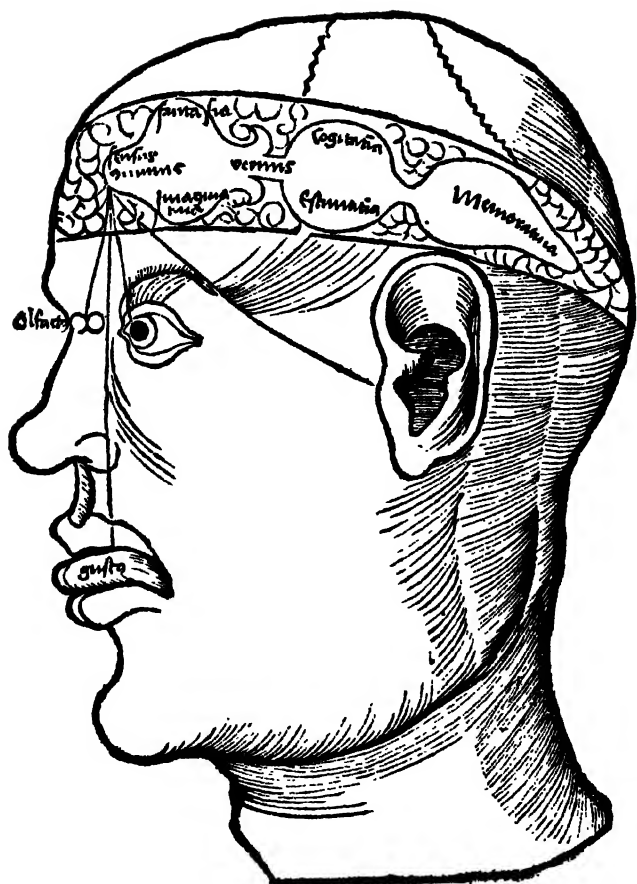
xiv. The *sensus communis* and the localised functions of the
brain. From G. Reisch, *Margarita Philosophiae*, Heidelberg,
1504.
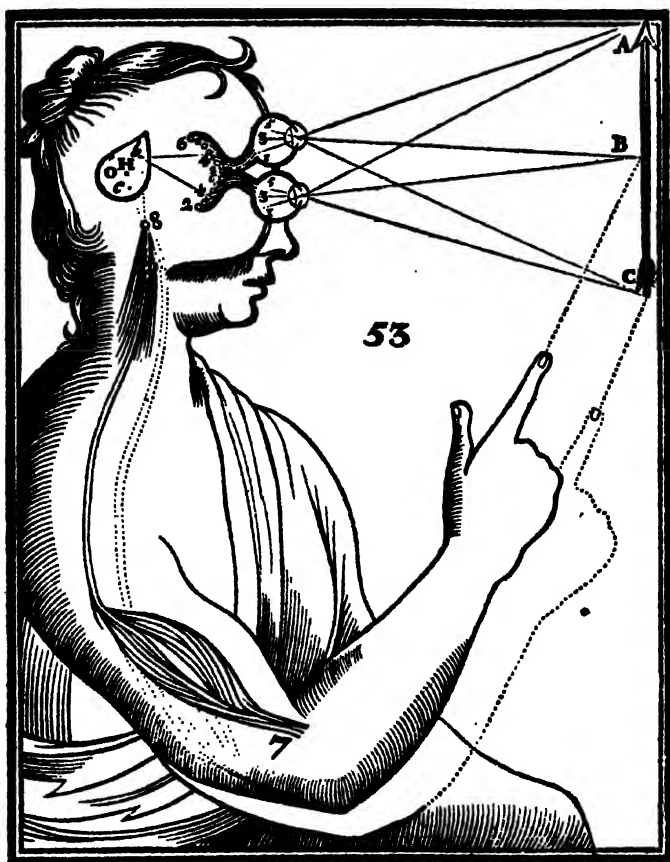
xv. Descartes' theory of perception, showing the transmission of the nervous impulse from the eye to the pineal gland and thence to the muscles. From *De Homine*, Amsterdam, 1677 (1st ed. Lyons, 1662).
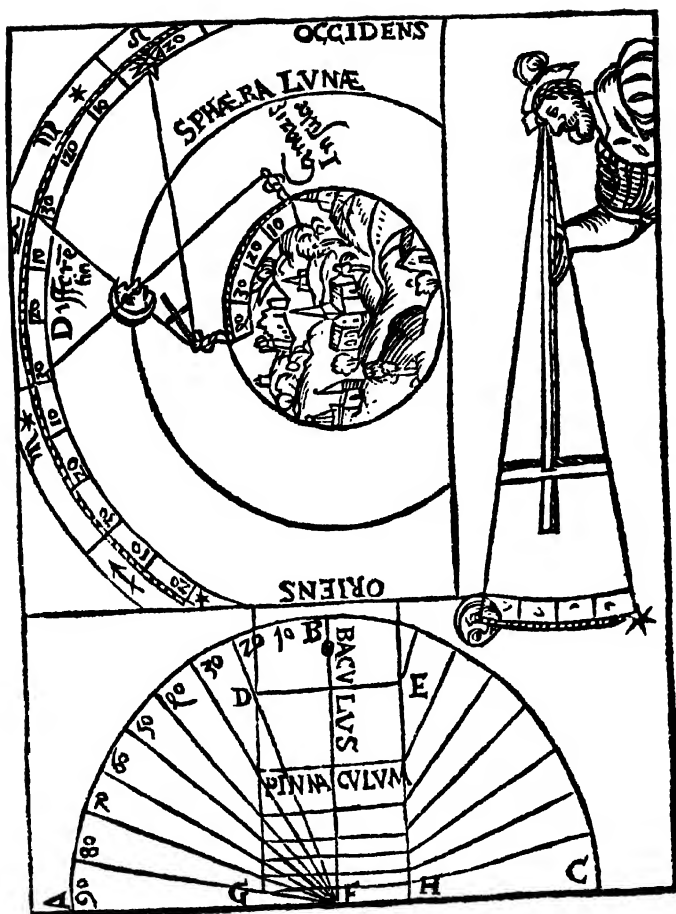
XVI. A cross-staff in use for surveying. From Petrus Apianus, Cosmographia, Antwerp, 1539.
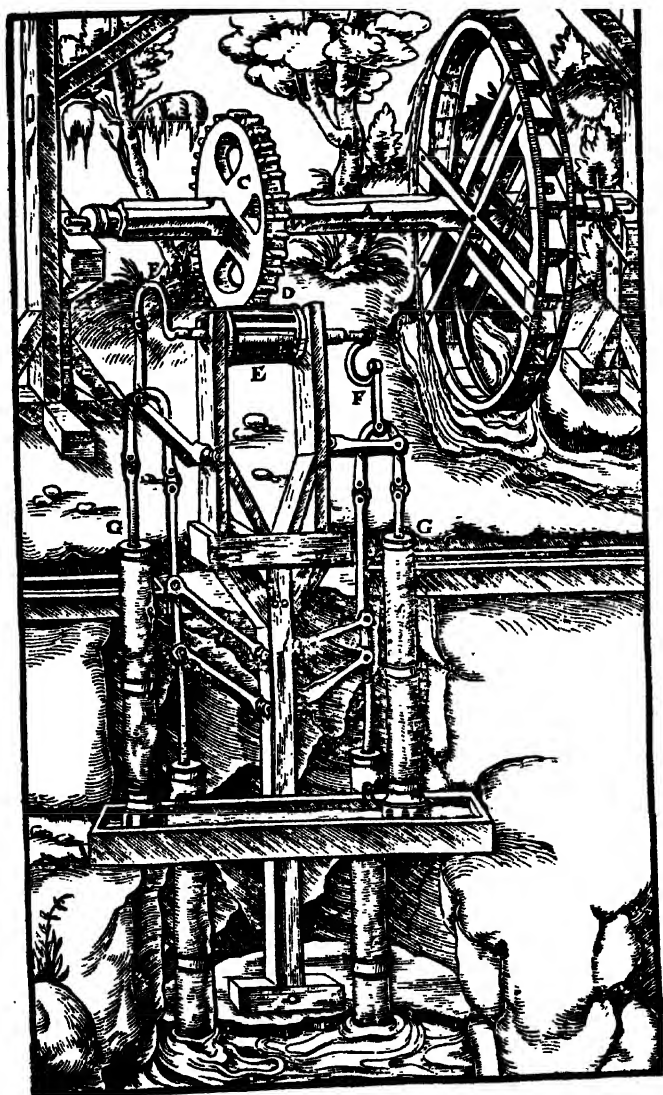
XVII. A water-driven suction pump in use at a mine. From
Agricola, *De Re Metallica*, Basel, 1561 (1st ed. 1556).
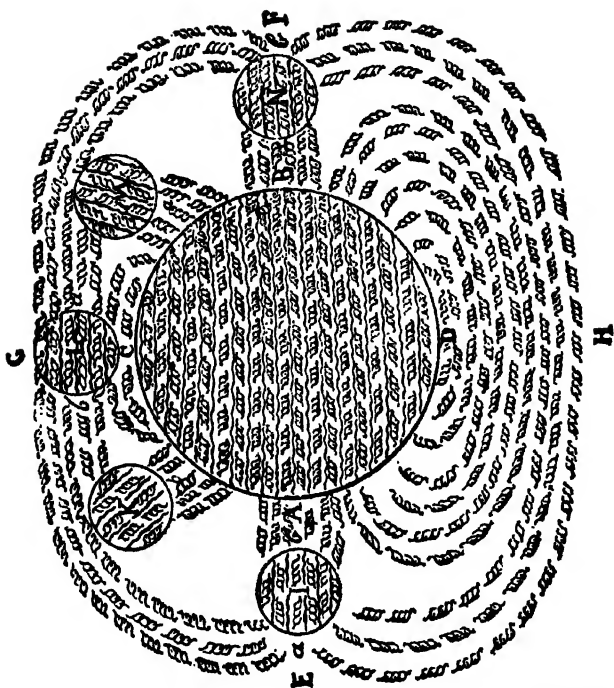
XVIII. Diagram from Descartes, Principia Philosophiæ (1644), illustrating his explanation of magnetism. He supposed that streams of screw-threaded particles passed through threaded passages in the earth and in iron, thus causing the alignment seen in the effect of a magnet on a piece of iron or of the earth on a compass needle.

xix. Botanists drawing plants. From Fuchs, *De Historia Stirpium*, Basel, 1542.

xx. Leonardo's drawing of the head and eye in section (cf. Plate xiv.). From *Quaderni D'Anatomia* 5, Royal Library, Windsor, MS by Gracious permission of H.M. the Queen.
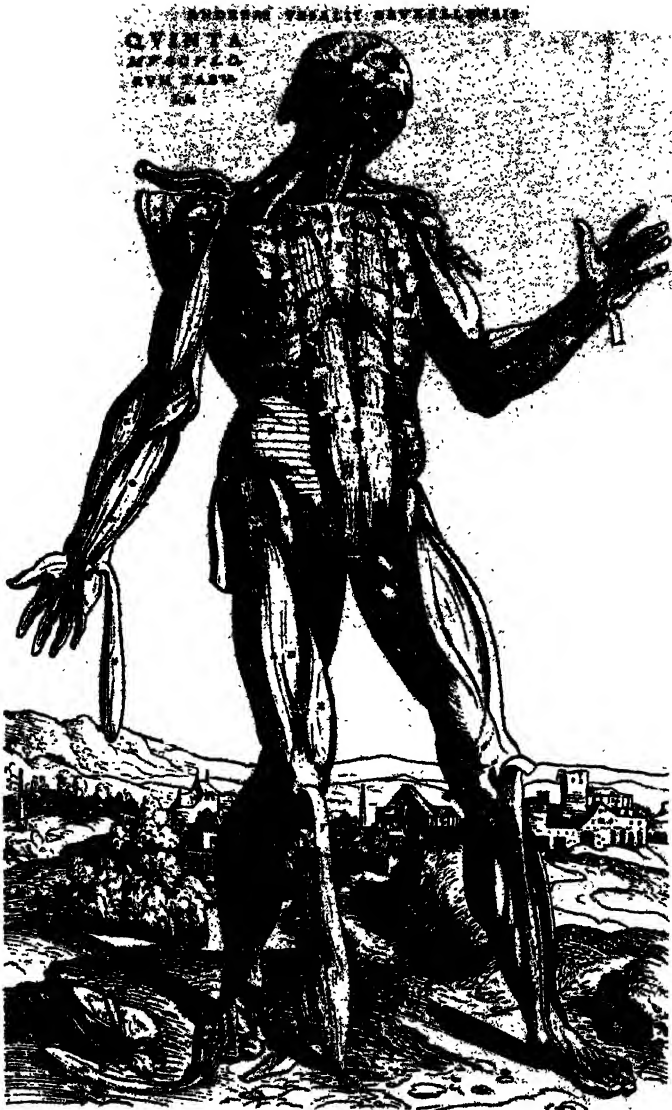
XXI. A dissection of the muscles. From Vesalius, *De Humani Corporis Fabrica* (1543).
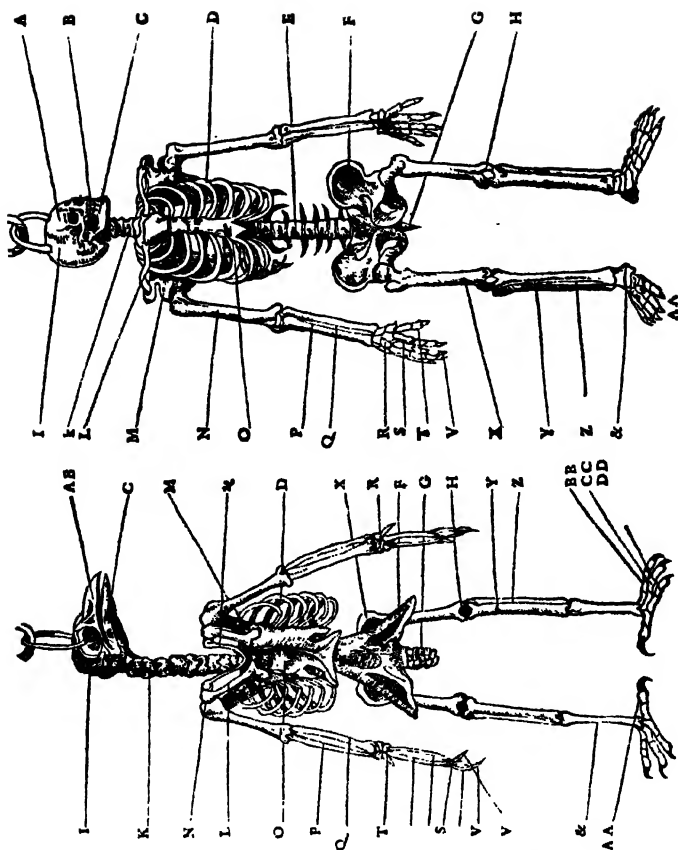
xxii. Diagrams illustrating the comparison between the skeletons of a man and a bird, from Belon, *Histoire de la nature des oyseaux*, Paris, 1555.
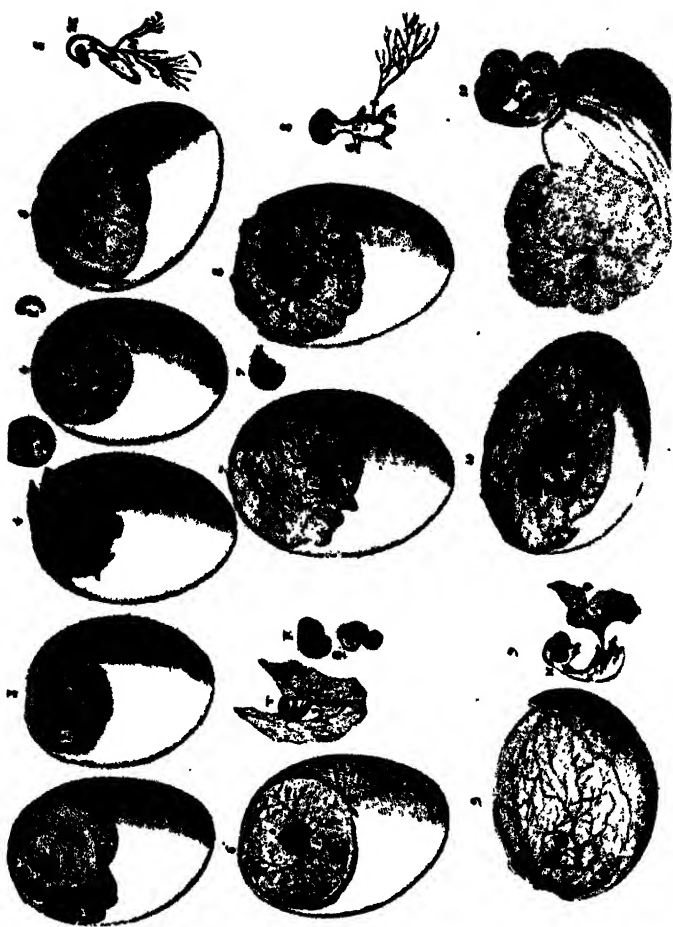
XXIII A. Embryology of the chick. From Fabrizio, *De Formatione Ovi et Pulli*, Padua, 1621.
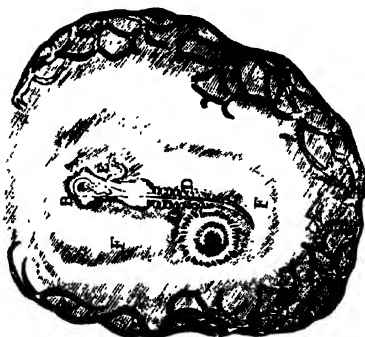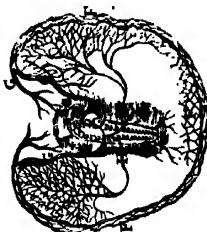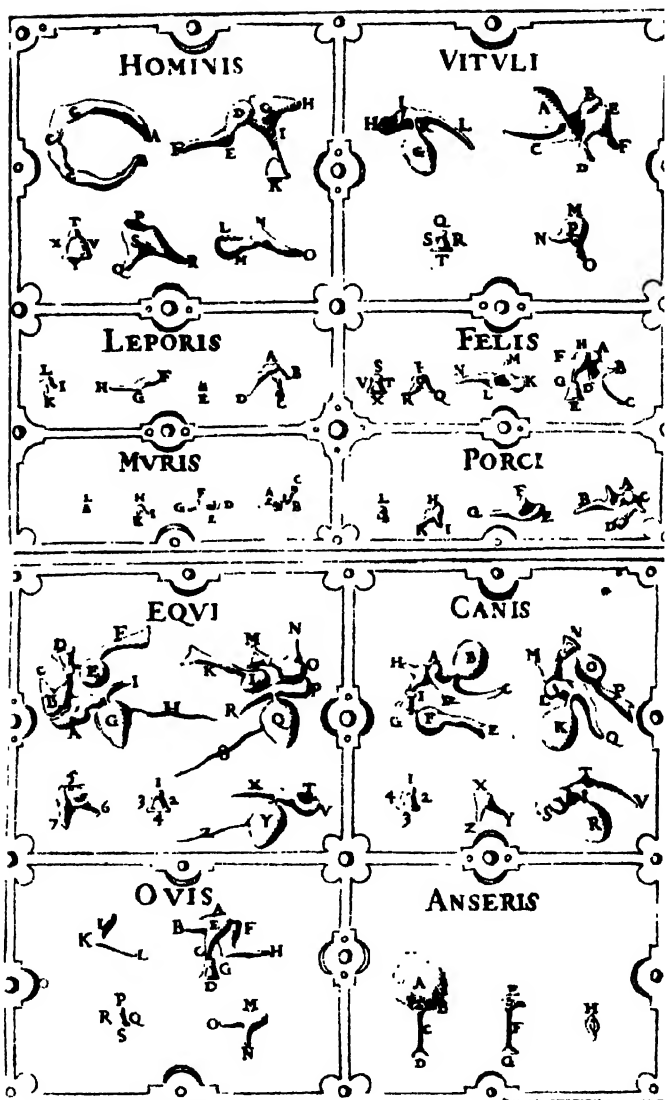
xxiv. The comparative anatomy of the ear ossicles from Casserio, *De Vocis Auditisque Organis*, Ferrara, 1601.

and Galileo made not only to answer the traditional ob-
jections to the earth's motion but also to produce conclu-
sive arguments in favour of it failed to convince most of
their contemporaries. For example, the magnetic chains
adopted by Kepler to explain the motion of the moon
would have made all movement of projectiles impossible.
Galileo was better placed for the negative operation of
meeting objections to the earth's motion. For example,
with his conceptions of *impeto* and of the composition of
impeti he was able to show that the argument from 'de-
tached bodies' lost its premisses. In his *Dialogue Concern-
ing the Two Principal Systems of the World, the Ptole-
maic and the Copernican* (a revealing title indicating his
indifference to Tycho Brahe and to Kepler) he pointed out
that such bodies would retain the velocity received from
the rotating earth unless forced to do otherwise. The re-
maining mechanical objection to the 'Copernican' theory
was from 'centrifugal force.' Galileo argued that this de-
pended, not on the linear velocity of a point on the earth's
surface, but on the angular velocity of rotation, and there-
fore that it was no greater on the earth's surface than on a
smaller body rotating once in the 24 hours. This would be
negligible compared with gravity. Actually centrifugal force
depends on both the linear and the angular velocity, as Huy-
gens was the first to show. Although the proof of the earth's
motion remained one of the principal goals of Galileo's dy-
namical work, in the end he was unable, in spite of all his
determined efforts, to do more than show that this was at
least as plausible as the supposition that it was at rest.

It was by an explicitly unrestricted and universal com-
parison of bodies on the earth with those in the heavens
that Newton, with the indispensable aid of some interme-
diate writers, finally produced the synthesis of his *Principia
Mathematica* (1687). Newton united Galileo's kinematic
laws of falling bodies and projectiles and the completion
of his principle of inertia, with Kepler's descriptive laws
of planetary motion and the completion of his conception
of gravitation (cf. above, pp. 155–66). Comparing a planet
with a projectile, he was then able to attribute the forward
motion of each to inertia, and the deflection from a rec-

tilinear trajectory to gravitation. A planet was thus a projectile whose velocity prevented it from falling on to the earth, so that its orbit formed an ellipse instead of a parabola.[10] Newton showed that the acceleration of fall of the moon in its elliptical orbit round the earth was equal to that required by Galileo's law of acceleration of free fall; the same applied to the planets' orbits round the sun. He deduced Kepler's Third Law from his inverse-square law of universal gravitation.[11] He showed that it was dynamically impossible for the enormous sun to turn round the diminutive earth, but that a central body and its satellite must revolve round their common centre of gravity, which in the solar system was inside the surface of the sun. Thus he was able to succeed where Galileo and Kepler had failed, not merely in refuting the arguments against the earth's movement, but in showing that the arguments in favour of it were compelling. They were compelling within a universal system of dynamics confirmed in all other tested fields of observation. For the first time since, in Hellenistic times, the observations had forced astronomers to abandon Aristotle's concentric spheres in favour of the physically inexplicable mathematical devices of epicycles and eccentrics and had produced the dichotomy between the physical explanation of the celestial motions and the mathematical means of predicting them—the dichotomy between Aristotelian physical cosmology and Ptolemaic mathematical astronomy that had existed throughout the Middle Ages—a conclusive criterion became available for choosing one calculating system in preference to another that was equally accurate in making predictions within the limits of astronomy. The choice operated by showing that only one of the alternative systems was compatible with a wider field of observations. It was Newton's achievement to put the dynamical criterion, foreseen and prepared by Galileo and Kepler, into operation, and to unite for the first time the explanation with the means of prediction. Beginning with the same fundamental physical axioms of

This orbit was achieved experimentally with the launching of Sputnik on 4 Oct.

And *vice versa*; see below, p. 208, note.

the laws of motion and of gravitation, the steps followed in setting out the *explanation* of the motions of bodies became exactly those made in *predicting* their motions. So cosmology as a science of 'natures' independent of calculation and the means of prediction disappeared within a genuine synthesis of mathematical-physics (cf. Vol. I, pp. 68 *et seq.*, 86 *et seq.*; also below, p. 300 *et seq.*).

Another difficulty remaining for the heliocentric system from the side of observation, and one which Galileo had been unable to solve, was the absence of stellar parallax. This was observed for the first time in 1838 by F. W. Bessel in the star 61 Cygni, though James Bradley, when looking for parallax, had observed, in 1725, that the fixed stars described small ellipses within exactly the duration of the terrestrial year, and that stars from the poles of the ecliptic to the ecliptic described figures which were increasingly less circular and more approaching straight lines. This was convincing evidence for the movement of the earth in an ellipse round the sun, but Bradley recognised that what he had observed were not parallactic ellipses, but aberrational ellipses due to the earth's approach, on one side of its orbit, to the light coming from the stars, and recession from it on the other.

It was because of his vision of a unified mathematical-physical cosmology that Galileo came into philosophical conflict with certain contemporary theologians; the other aspects of his troubles with the Roman Inquisition and the course of his trial belong rather to the history of Roman ecclesiastical policy and judicial procedure—in this case obscure enough—than to the history of science. Yet it is significant that it was a theological problem, the relationship between astronomical theory and Scripture, between the cosmology discovered by scientific reasoning and that presented as revealed by God, that should have made the truth of the 'realist' view of science shared by Galileo and Kepler the great question of the day in the philosophy of science. Oresme had already considered and then withdrawn before the same passages of Scripture, which must be *literally* false if the new cosmology were literally and physically true (see above, p. 81 *et seq.*). For example

Joshua's command on the evening of the battle of Gibeon: 'Sun, stand thou still upon Gibeon; and thou, Moon, in the valley of Ajalon; and the Sun stood still, and the moon stayed . . .' (Joshua, x, 12, 13), implied that the sun was normally in motion. Other passages contradicted the other essential Copernican postulate, that the earth moved: for example, from Psalm 93, 'the world also is stablished, that it cannot be moved.' Granting the various mathematical and practical advantages of the new astronomy, as everyone was prepared to, there were two ways of avoiding this conflict. One was to abandon the literal interpretation of Scripture, a course that had been followed, albeit with proper caution, by the Fathers themselves when the occasion had called for it. The other way was to weaken the truth of natural science, to treat astronomical theory not as a discovery of the real physical world, a world of abstract laws perhaps but knowable as true, but as a convenient fiction for making the calculations, 'merely a poetical conceit, a dream,' 'a chimæra,' as Galileo wrote ironically in a letter to Leopold of Austria in 1618.

After some preliminaries, Galileo finally stated his position publicly in 1615 in his open *Letter to Madame Cristina of Lorraine, Grand Duchess of Tuscany*, written on the advice of some clerical friends, partly to clear himself of a malicious rumour that he was an unbeliever, and also to try, unsuccessfully, to prevent the ecclesiastical authorities from making the fatal mistake of condemning the Copernican system on theological grounds. Citing the authority of St. Augustine, Galileo argued that God was the author not only of one great book but of two, of Nature as well as the Scriptures. Truth was to be studied in both, but with different results. The book of nature was to be read in the language of mathematical science and the results expressed in physical theory; the Scriptures, on the other hand, contained no physical theory, but revealed to us our moral destiny. When they referred to natural phenomena they used the language of ordinary understanding, conforming to popular ideas, without implying that their literal meaning was to be taken as referring to physical fact. Indeed he pointed out that the Scriptures had always

been understood to use figurative language at many points, as when they mentioned the eye or hand or anger of God, where a literal interpretation would be directly heretical. It was against both reason and tradition to use a literal interpretation of Scriptures to throw doubt upon the truth of statements expressing either the direct evidence of the senses or necessary conclusions from that evidence.

'It seems to me,' Galileo wrote in his *Letter to the Grand Duchess*, 'that in discussing natural problems we ought not to start from the authority of the texts of the Scriptures, but from the experience of the senses and from necessary demonstrations (*dalle sensate experienze e dalle dimostrazione necessarie*). For, Holy Scripture and Nature alike proceeding from the Divine Word, the former as the dictation of the Holy Ghost, the latter as the most observant executrix of God's commands; and moreover, it being convenient in the Scriptures (by way of condescension to the understanding of all men) to say many things different, in appearance and so far as concerns the naked signification of the words, from the absolute truth; but nature, on the other hand, being inexorable and immutable and never passing beyond the bounds of the laws assigned to her, as if she did not care whether her abstruse reasons and modes of operation were or were not within the capacity of men to understand; it is clear that those things concerning natural effects which either the experience of the senses sets before our eyes or necessary demonstrations prove to us, ought not to be called in question on any account, much less condemned on the basis of texts of Scripture which may, in the words used, seem to mean something different. For every expression of Scripture is not tied to strict conditions like every effect of nature; nor does God reveal himself less admirably in the effects of nature than in the sacred words of Scripture.'

Clearly, he concluded, it was not the intention of the Holy Ghost to teach us physics or astronomy, or to show us whether the earth moved or was at rest. These questions were theologically neutral, although certainly we should respect the sacred text, and where appropriate use the conclusions of science to help to discover its meaning. The

purpose of the Holy Ghost in the Scriptures, as he expressed it wittily in a remark which he attributed to Cardinal Baronio, was to teach us 'how to go to heaven, not how the heavens go.'

'This granted,' he continued, 'and it being true, as has been said, that two truths cannot be contrary to each other, it is the office of a judicious interpreter to try to penetrate to the true senses of sacred texts, which undoubtedly will agree with those natural conclusions which manifest sense and necessary demonstrations have first made sure and certain. Indeed, it being the case, as has been said, that the Scriptures, for the reasons stated, admit in many places of interpretations far from the sense of the words; and, moreover, we not being able to affirm that all interpreters speak by divine inspiration (for, if it were so, then there would be no diversity between them concerning the meanings of the same texts); I should think that it would be an act of great prudence to forbid anyone to usurp the texts of Scripture and as it were to force them to maintain this or that natural conclusion for truth, of which the senses and demonstrative and necessary reasons may one time or another assure us the contrary. For who will prescribe bounds to man's intelligence and invention? (*E chi vuol por termine alli umani ingegni?*) Who will assert that all that is sensible and knowable in the world is already discovered and known? Perhaps those who on other occasions avow (and with great truth) that *ea quæ scimus sunt minima pars eorum quæ ignoramus* [those things that we know are very few in comparison with those we do not know]. Indeed, if we have it from the mouth of the Holy Ghost himself that *Deus tradidit mundum disputationi eorum, ut non inveniat homo opus quod operatus est Deus ab initio ad finem* [God offers the world for their disputation, so that no man may find out the work that he makes from the beginning to the end—*Ecclesiastes*, iii, 11], we ought not, as I conceive, contradicting such a sentence, stop the way to free philosophising about the things of the world and of nature, as if they were already certainly found and all clearly known.'

A man of the world as well as a convinced Catholic and a

dedicated natural philosopher, a guest cherished at aristo-
cratic tables for his genial intelligence and witty conversa-
tion, Galileo knew well the weight that political decisions,
both ecclesiastical and secular, attached, of their nature,
to convenience and administrative peace. With a prophetic
foresight into his own future troubles he pointedly em-
phasised the distinction between the conditions for a
change of legal or commercial and of scientific opinion.[12]
'I would entreat those wise and prudent Fathers that they
should with all diligence consider the difference that exists
between demonstrative knowledge and knowledge where
opinion is possible: to the end, that weighing well in their
minds, with what force necessary conclusions compel ac-
ceptance, they may the better ascertain for themselves that
it is not in the power of those who profess the demonstra-
tive sciences to change their opinions at pleasure and to
apply themselves now on one side and now on the other;
that there is a great difference between commanding a
mathematician or a philosopher and disposing of a mer-
chant or a lawyer; and that the demonstrated conclusions
touching the things of nature and of the heavens cannot
be changed with the same facility as opinions about what
is legal or not in a contract, rent, or bill of exchange.'
   On the basis of the observations and of the new dy-
namics Galileo believed that it would be possible to dem-
onstrate that the heliocentric system was a necessary con-

----

[12] Cf. Francis Bacon, *Advancement of Learning* (1605): 'Yet to
those that seek truth and not magistrality, it cannot but seem a
matter of great profit, to see before them the several opinions
touching the foundations of nature: not for any exact truth that
can be expected from those theories; for as the same phenomena
in astronomy are satisfied by the received astronomy of the diur-
nal motion, and the proper motions of the planets, with their
eccentrics and epicycles, and likewise by the theory of Copernicus,
who supposed the earth to move (and the calculations agree indif-
ferently to both), so the ordinary face and view of experience is
many times satisfied by theories and philosophies; whereas to find
the real truth requireth another manner of severity and attention.'
He added: 'So many say that the opinion of Copernicus touching
the rotation of the earth, which astronomy itself cannot correct,
because it is not repugnant to any of the phenomena, yet natural
philosophy may correct.'

clusion from the data. He had seen with his telescope a model of the solar system in Jupiter and his satellites, and he had measured the great annual variation in the apparent diameters of Venus and Mars. His observations of the phases of Venus had confirmed, so far as he had gone, the prediction from the Copernican system that the inner planets, and they alone, would show complete phases like the moon when observed from the earth (cf. Fig. 6). There were, he said, 'many other sensible observations which can never by any means be reconciled with the Ptolemaic system, but are most weighty arguments for the Copernican.' There were some natural propositions of which human science and discourse could furnish us only rather with 'some probable opinion and plausible conjecture, than with any certain and demonstrated knowledge.' But 'there are others, of which either we have or we may confidently believe that it is possible to have, by experiments, prolonged observations and necessary demonstrations, an indubitable certainty; as for instance, whether the earth or the sun move or not, and whether the earth is spherical or otherwise.'

If the Copernican theory, or the particular opinion of the earth's mobility, were prohibited and declared contrary to the Catholic faith without prohibiting astronomy as a whole, Galileo continued his earnest advocacy, it could only cause great scandal. It could only be to the detriment of souls 'to give them occasion to see a proposition proved which it would afterwards become a sin to believe. And what other would the prohibiting of the whole science be than an open contempt of a hundred texts of the Holy Scriptures, in which we are taught that the glory and the greatness of Almighty God are admirably discerned in all his works, and divinely read in the open book of heaven?' It would be to contradict all the evidence of God's intention in endowing man with his admirable intelligence and inquiring reason. Galileo warned theologians against putting the faithful in the embarrassing position of having to believe as true what their senses, and scientific demonstrations, might show them to be false, or of committing a sin when they believed what their reason convinced them

to be true. Moreover he pointed out that even the geo-static system disagreed with the literal words of Scripture. For example, if Joshua's command to the sun was meant to have been taken literally, according to this system he should have addressed it to the Prime Mover, for by stopping the sun and moon alone he would have deranged the whole celestial system, yet there is no evidence that he did so. The association of Aristotelian cosmology and Ptolemaic astronomy with the language of theology was not only purely accidental but far from complete.

Galileo wrote in the language of uncompromising scientific realism. He believed in an objective world of unchanging law existing independently of the inventions of men, a true world which it is the business of science to discover, to be sure by subtle theoretical reasoning, but nevertheless with certainty. 'Nothing ever changes in nature to accommodate itself to the comprehensions or motions of men,' he wrote to his friend Elia Diodati in 1633. While dedicated to a mathematical approach to the natural world, he agreed with the medieval 'physicists' rather than the 'mathematicians' in astronomy, and was not content to stop simply at 'saving the phenomena.' Like Aquinas he presupposed a true physical theory, a real physical substance causing the phenomena (cf. Vol. I, pp. 82–83). But if the real physical world was an abstract structure of the real mathematical 'primary qualities' and their laws, qualities determining the nature of physical substance, then the system of theories stating these laws must necessarily be formulated consistently throughout the entire range of physical phenomena according to uniform mathematical principles. It was precisely the discontinuities in the existing science of motion, for example between Ptolemaic astronomy and Aristotelian cosmology and between the qualitatively different kinds of motion within the latter, that Galileo found so unsatisfactory. It was quite true, as Salviati said in the Third Day of the *Two Principal Systems*, that 'the principal aim of pure astronomers is to give reasons only for the appearances in celestial bodies, and to fit to these and to the motion of the stars such structures and compositions of circles that the mo-

tions following from these calculations correspond with those same appearances, having few scruples about admitting anomalies which might in fact prove troublesome in other respects.' But a criticism he made of the Ptolemaic system was just that 'although it satisfied an astronomer merely arithmetical (*puro calcolatore*), yet it did not afford satisfaction or content to the astronomer philosophical,' that is, who was also a natural scientist. But, he added, Copernicus 'had very well understood that if one might save the celestial appearances with assumptions false in nature, it might much more easily be done with true suppositions.'

The characteristic of Galileo's philosophy of science that came to dominate his side of the controversy over the Copernican theory was the particular form of his conviction that his new mathematical science was a method of reading the real book of nature It was his belief that 'natural propositions' could be 'demonstrated necessarily,' that the experimental verification of a theory could establish it with 'indubitable certainty.' Describing the opening of an investigation by means of an 'hypothetical assumption,' he said in *Two New Sciences* that this could be accepted conditionally 'as a postulate, the absolute truth of which will be established when we find that the inferences from it correspond to and agree with experiment' He used such language not only when establishing the kinematic law of free fall as a fact, but also in speaking of the Copernican theory. So, when he repeated the argun ent that this was more economical than the Ptolemaic theory, he was not using it in any conventionalist sense. It was Nature herself that 'does not do that by many things, which may be done by few,' as he said in the Second Day of his *Two Principal Systems*.

Apparently not clearly distinguished in his own mind from this conviction that irrefutable verification was possible in science, Galileo's fundamental contribution to the cosmological debate was to see that a new and precise physical criterion, such as had long been accepted as appropriate to decide between rival mathematical theories in astronomy (see Vol. I, p. 86 *et seq.*), was at hand in the

new inertial dynamics. Treating all motion, celestial and terrestrial alike, as explicable by a single system of dynamics, he wanted to unite, in this system, the explanation with the means of prediction of the various motions. In the law of inertia he saw the possibility of a higher theory with which the geocentric theory was incompatible and only the heliocentric theory compatible. He failed in his own attempt to use this dynamical criterion because he failed both to generalise the law of inertia completely and to appreciate the true geometry of the heliocentric system as set out by Kepler, but it was by means of this criterion that the decision was eventually made.

But in 1615 Galileo had not yet begun to stress the dynamical argument for the Copernican theory, and it was rather with the difficulty of establishing necessary truths about the things of experience in any particular case that the principal actor on the ecclesiastical side of the debate made his riposte. This was Cardinal Robert Bellarmine (1542–1621). A student of astronomy in his youth, it had been Bellarmine's unhappy task to frame the decision that led Giordano Bruno to his death at the stake in 1600.[13] Undoubtedly his policy over Galileo was based on a determination never to let that episode be repeated. Over seventy years old, he aimed at administrative peace, and his method of achieving it was to take the alternative way to Galileo's in order to escape the conflict between astronomy and Scripture. His policy was to weaken the conclusions of natural science and to accept the new astronomy as in no sense established with 'indubitable certainty' but only as 'probable opinion and plausible conjecture,' to accept it only in a form that would leave undisturbed the literal interpretation of Scripture and the Aristotelian cos-

[13] It seems that Bruno was not charged with his advocacy of the Copernican system. According to Lynn Thorndike, *History of Magic and Experimental Science*, vol. 6, p. 427: 'Except that on March 24, 1597, he was admonished to give up such idle notions of his as that of a plurality and infinity of worlds, what counted most against him was his apostasy from his Order, his long association with heretics, and his questionable attitude as to the Incarnation and Trinity.'

mology which historical accident had married with it. He
shut his eyes to the respects in which the union was less
like marriage than living in sin. Yet although primarily ad-
ministrative in their aim and limited in their application,
it cannot be denied that Bellarmine's arguments succeeded
in making a philosophical point against Galileo. Their two
philosophies represent a classical polarisation of opposites,
an antithesis in the conception of the discoveries and in-
ventions of theoretical science that is at once ancient, per-
sistent, and easily misunderstood.

The principle had been well known to scholastic logi-
cians, that the phenomena cannot uniquely determine the
hypotheses that must 'save,' or explain them, for the same
conclusions can be deduced from very different premisses,
and the experimental verification of the consequent does
not enable us to affirm the antecedent. This principle, de-
veloped at Oxford in the 13th and 14th centuries, had
been a commonplace of the logical school of Padua in the
early 16th century (cf. above, p. 25 et seq.). A typical
statement is that by Agostino Nifo. In his commentary on
Aristotle's *Physics*, Nifo had distinguished between the log-
ical processes of discovery and of demonstration, and had
contrasted the certainty of mathematics, where premisses
and conclusion are reciprocating, with the conjectural
character of our knowledge of causes in natural science.[14]
Going on to consider astronomical hypotheses, Nifo wrote
in his *De Caelo et Mundo Commentaria*, published at
Venice in 1553, book 2:

> In a good demonstration, the effect necessarily follows
> from the assumed cause, and this must necessarily be
> assumed in view of the observed effect. Now the eccen-
> trics and epicycles being admitted, it is true that the
> appearances are saved. But the converse of this is not
> necessarily true, namely that given the appearances, the
> eccentrics and epicycles must exist. This is true only pro-
> visionally until a better explanation is discovered which

[14] Many scientists, including Descartes and Newton, have
shared the ideal of trying to make natural science as nearly as pos-
sible like mathematics in this respect; cf below, pp. 307, 324

both necessitates the phenomena and is necessitated by them. Accordingly those men are in error who, taking a natural phenomenon, the occurrence of which might flow from many causes, conclude in favour of one cause.

The occasion that led Bellarmine to use this logical doctrine to draw the teeth of Galileo's arguments for the new astronomy was a letter written by a countryman of Galileo's, the Carmelite friar Paolo Antonio Foscarini, who had followed Galileo in suggesting that the Copernican system should be considered as a physical truth, not as a mere calculating device, and had shown how the relevant passages of Scripture could be reconciled with it. Bellarmine's reply, also written in 1615, rejected Foscarini's proposal.

'It seems to me,' he wrote, 'that your Reverence and Signor Galileo act prudently when you content yourselves with speaking hypothetically (*ex suppositione*) and not absolutely, as I have always understood that Copernicus spoke. To say that on the supposition of the Earth's movement and the Sun's quiescence all the celestial appearances are explained better than by the theory of eccentrics and epicycles [!], is to speak with excellent good sense and to run no risk whatever. Such a manner of speaking is enough for a mathematician. But to want to affirm that the Sun, in very truth, is at the centre of the universe and only rotates on its axis without going from east to west, and that the earth is in the third heaven [i.e., sphere—see Pl. VI] and revolves with the greatest speed round the sun, is a very dangerous attitude and one calculated not only to arouse all scholastic philosophers and theologians but also to injure our holy faith by contradicting the Scriptures. Your Reverence has clearly shown that there are several ways of interpreting the Word of God, but you have not applied these methods to any particular passage; and, had you wished to expound by the method of your choice all the texts which you have cited, I feel certain that you would have met with the very gravest difficulties.

'As you are aware, the Council of Trent forbids the interpretation of the Scriptures in a way contrary to the common opinion of the holy Fathers. . . . It will not do to say

that this is not a matter of faith, because though it may not be a matter of faith *ex parte objecti* or as regards the subject treated, yet it is a matter of faith *ex parte dicentis*, or as regards him who announces it. . . .

'If there were a real proof that the Sun is in the centre of the universe, that the Earth is in the third heaven, and that the Sun does not go round the Earth but the Earth round the Sun, then we should have to proceed with great circumspection in explaining passages of Scripture which appear to teach the contrary, and rather admit that we did not understand them, than declare an opinion to be false which is proved to be true. But, as for myself, I shall not believe that there are such proofs until they are shown to me. Nor is it a proof that, if the Sun be supposed at the centre of the universe and the Earth in the third heaven, the celestial appearances are thereby saved, equivalent to a proof that the sun actually is in the centre and the Earth in the third heaven. The first kind of proof might, I believe, be found, but as for the second kind, I have the gravest doubts, and in the case of doubt we ought not to abandon the interpretation of the sacred text as given by the holy Fathers.'

Evidently Bellarmine had not mastered the technical details of *De Revolutionibus*, but he had read Osiander's cautious preface. The Copernican system was to be treated simply as a mathematical hypothesis for making calculations; it had been used as such for the Gregorian calendar in 1582. Galileo's views on the interpretation of Scripture, explicitly an exposition of the doctrines of St. Augustine and the Fathers, were in themselves well received in Rome. The only query was the prudence of a layman setting out to teach the theologians their business. But it was Bellarmine's philosophical policy, the policy of Osiander, a Lutheran pastor, that prevailed in the deliberations of the Congregation of the Holy Office, before which the Copernican affair had come. No doubt the Roman authorities were partly concerned to preserve the text of Holy Scripture against private interpretations on the Protestant model, to which there seemed no limit. In any case they played for safety. Galileo's personal intervention in Rome

failed to convince anyone that the Copernican theory was physically true, though it was useful in clearing him personally of a quite unfounded and maliciously inspired suspicion of heresy and blasphemy. On 24 February 1616 the theological experts, or Qualifiers, of the Holy Office delivered their famous report. They reported that the proposition that 'the sun is the centre of the world and altogether devoid of local motion' was 'foolish and absurd philosophically, and formally heretical, in as much as it expressly contradicts the doctrines of Holy Scripture in many places, both according to their literal meaning, and according to the common exposition and meaning of the holy Fathers and Doctors'; and that the proposition that 'the earth is not the centre of the world nor immovable, but moves as a whole. and also with a daily motion' was worthy 'to receive the same censure in philosophy and, as regards theological truth. to be at least erroneous in faith.'

On 3 March the Congregation of the Index issued its decree prohibiting Copernicus' *De Revolutionibus* until it had been corrected. Partly because of the intervention of Cardinal Maffeo Barberini. the future Pope Urban VIII, the Congregation made a distinction between scientific hypothesis and theological interpretation, and refused to prohibit *De Revolutionibus* altogether. The 'corrections' amounted to very minor changes, but made it appear that it was presenting only an hypothesis. In 1620 the book was once more permitted to be read. Moreover, the prohibition was not issued in such a way that the Copernican theory ever became formally heretical, although many contemporaries, unaware of the niceties of the distinction, understandably thought it was. The book by Foscarini on the interpretation of Scripture was at the same time prohibited absolutely. Galileo was not mentioned by name, although he was really the central character in the drama and the principal victim. Nothing if not forthright. he had been unsparing in his advocacy of the new astronomy during the whole of that Roman winter. 'We have here Sig. Galileo, who often, in gatherings of men of curious mind. bemuses many concerning the opinion of Copernicus which he holds for true,' wrote a certain urbane Monsignor Querengo (in

a letter included in a National Edition of Galileo's Works, published in Florence). 'He discourses often amid fifteen or twenty guests who make hot assaults upon him, now in one house, now in another. But he is so well buttressed that he laughs them off; and although the novelty of his opinion leaves people unpersuaded, yet he convicts of vanity the greater part of the arguments with which his opponents try to overthrow him. On Monday in particular, in the house of Federico Ghisilieri, he achieved wonderful feats; and what I liked most was that, before answering the opposing reasons, he amplified them and fortified them himself with new grounds which appeared invincible, so that in demolishing them subsequently he made his opponents look all the more ridiculous.' Certainly the simple facts of personalities were an important influence in this drama on which so much philosophical ink has been spilt. After the decree Querengo wrote again expressing the view of the uncommitted man of the world. 'The disputes of Signor Galileo have dissolved into alchemical smoke, since the Holy Office has declared that to maintain this opinion is to dissent manifestly from the infallible dogmas of the Church. So here we are at last, safely back on a solid Earth, and we do not have to fly with it as so many ants crawling around a balloon.'

There are two documents alleging to describe what was said to Galileo after the Congregation of the Holy Office had reached its decision. According to a certificate given to him by Bellarmine, he was simply notified of the decree declaring that the Copernican theses were contrary to Scripture 'and consequently must not be either held or defended.' But according to a minute, possibly false, inserted into the Inquisition record of the proceedings, Galileo was warned by Bellarmine 'of the error of the aforesaid opinion and admonished to abandon it; and immediately thereafter' was ordered by the Commissary General of the Holy Office, in the presence of Bellarmine and other witnesses, 'in the name of His Holiness the Pope and the whole Congregation of the Holy Office, to relinquish altogether the said opinion that the Sun is the centre of the world and immovable and that the Earth moves; nor further to hold,

teach or defend it in any way whatsoever, verbally or in writing; otherwise proceedings would be taken against him by the Holy Office; which injunction the said Galileo acquiesced in and promised to obey.' The difference between these two versions was to become material in Galileo's trial in 1633.

Galileo waited for an opportunity to prove an opinion he had good, though not conclusive, reasons for holding to be true. This came with the election in 1623 of Maffeo Barberini as Pope Urban VIII, a Florentine, a friend of the arts, and with Galileo a member of the Academia dei Lincei. Galileo had disposed of all the arguments put forward *against* the earth's motion. Moreover he came to the conclusion that only by assuming the double motion of the earth on its axis and round the sun was it possible to explain the ebb and flow of the tides. Disbelieving in attraction at a distance, he did not accept Kepler's gravitational theory. He proposed instead an explanation based on the conservation of the sea's momentum. His object was to show that the movements of the tides could be demonstrated from the assumption of the earth's daily and annual revolutions, and that the existence of those revolutions was demonstrated by the existence of the tides. It was this capital dynamical proof that eventually formed the culmination of the *Dialogue concerning the Two Principal Systems of the World* (1632), in the Fourth Day, to which all the preceding dynamical discussion led up. It did not carry much conviction with Galileo's contemporaries, but it was only with the later work of Huygens and Newton that it became possible to get to the bottom of the matter and to see the fallacy in Galileo's ingenious argument.

Galileo's hopes for a genuine reopening of the Copernican question were not fulfilled. Urban agreed to his publishing a further discussion of the subject only on condition that it should remain hypothetical. The ecclesiastical point of view had not in fact changed since the time of Bellarmine. Its difference from Galileo's can be measured by the speech at the end of the *Dialogue* in which Galileo put into the mouth of Simplicio the opinions with which

the Pope had instructed him to conclude. In discussing the assertion that it was possible to prove the motion of the earth conclusively, Simplicio asked whether God by his infinite power and wisdom could not have caused the tides by some other means than that envisaged by Galileo. 'Keeping always before my mind's eye a most solid doctrine that I once heard from a most eminent and learned person, and before which one must fall silent . . . ,' he declared, 'I know that you would reply that He could have, and that He would have known how to do this in many ways, which are beyond the reach of our minds. From which I forthwith conclude that, this being so, it would be an extravagant boldness for anyone to limit and confine the divine power and wisdom to some particular fancy (*fantasia particolare*) of his own.' Salviati responds: 'An admirable and truly angelic doctrine, and well in accord with another one, also divine, which, while it grants to us the right to argue about the constitution of the universe (perhaps in order that the working of the human mind shall not be curtailed or made lazy) adds that we cannot discover the work of His hands.'

The argument from God's omnipotence that had been used to free natural science from the restrictions of Aristotelianism in the 13th century had proved a boomerang.[15] Galileo's point of view was that while this argument was undoubtedly true, he was interested in discovering the way God had *actually* acted in making the world.

[15] Cf. Leibniz's letter to the Abbé Conti, Nov. or Dec. 1715, referring to Newton's natural theology, on which he was engaged in a controversy with Samuel Clarke: 'And because we do not yet know perfectly and in detail how gravity is produced or elastic force or magnetic force, this does not give us any right to make of them scholastic occult qualities or miracles; but it gives us still less right to put bounds to the wisdom and power of God and to attribute to him a *sensorium* and such things' (*Recueil de diverses Pièces sur la Philosophie, la Religion Naturelle, l'Histoire, les Mathématiques, etc. par Mrs. Leibnitz, Clarke, Newton, & autres Auteurs célèbres*, ed. Des Maiseaux, Amsterdam, 1720, ii, 9.) 'Credulity is hurtful, so is incredulity: the business therefore of a wise man is to try all things, hold fast to what is approv'd, never to limit the power of God, nor assign bounds to nature.' (Boerhaave, *A New Method of Chemistry*, London, 1741.)

Thus if he were to publish a proof of the Copernican theory at all without going directly against ecclesiastical authority, it was impossible for Galileo to avoid some prevarication. The general order contained in the decree of 1616 still stood. It was his miscalculation of the risk that led to his disaster, although it may be justly argued that this in no way justified the action that was taken against him. He took every precaution, aided by his friends, the Master of the Holy Palace, the chief official responsible for licensing, and the Pope's own secretary, to make sure that the *Dialogue* would appear with every proper official sanction. It received the *imprimatur* of the Archbishop of Florence, although there seems to have been some genuine confusion between the different authorities, all of them well disposed. Following the Pope's instructions Galileo had added a preface and a conclusion disclaiming that his arguments were anything more than probable and hypothetical. But since the whole burden of the discussion in the pages between preface and conclusion had the very opposite intention, the casuistry of this disclaimer was the more obvious. Urban, with some justice, accused Galileo of breaking a personal promise made to himself. The Roman Inquisition then charged him with disobeying the injunction recorded in the minute of 1616 and of only pretending to present the condemned opinion 'as an hypothesis' (*quamvis hypothetice*). Galileo denied any knowledge of the injunction. After proceedings that were anything but straightforward, he was found guilty, three out of the ten Cardinal judges withholding their signatures, and on 22 June 1633, in the Dominican Convent of Santa Maria Sopra Minerva, he was obliged to abjure his belief in the condemned Copernican theses. The *Dialogue* was prohibited. The earliest appearance of the famous phrase *Eppur si muove* seems to have been in the inscription on a portrait of Galileo painted in the year of his death. It is unlikely, after a submission so humiliatingly complete, that in fact he murmured these words on rising from his knees. As to his physical treatment during the trial, all the evidence shows that the worst he suffered was confinement in comfortable quarters. It was a more serious inconven-

ience to be confined for the rest of his life to his farm at Arcetri, in the hills to the south overlooking Florence. His real suffering was of a different kind. Experience had taught Galileo to distinguish between truth and the behaviour of those claiming to act in its interests. But it was almost past bearing to suffer humiliation at the hands of the authorities of the Church in whose doctrines he believed and whom it was his desire to serve. The triumph of 'ignorance, impiety, fraud and deceipt,' as he described the trial in later life, was as unnecessary as it was unwelcome a conclusion to the intelligent inquiries of Christian philosophers of science.

The decree against the Copernican theses and Galileo's condemnation placed Catholics in a false position for more than a century, without preventing excellent work being done in practical astronomy in Italy and other Catholic countries and the uninhibited development of other sciences there. Galileo himself, though an old man, went on with his work on mechanics and completed what was really his most important contribution to the subject, his *Discourses Concerning Two New Sciences*. But he had it published in Holland, in 1638. Even in theoretical astronomy excellent work went on behind the façade of ingenious quibbles. For example Alfonso Borelli, in 1660, observed the letter of the decree by limiting to Jupiter and his satellites the suggestive theory of celestial mechanics which he obviously intended to apply to the earth and the moon. Another curious result of the decree was the edition of Newton's *Principia* published in 1739–42, with a commentary by the Minim Fathers Le Seur and Jacqier presenting the Newtonian system of the world 'hypothetically'; the *Principia* had been announced originally in the *Philosophical Transactions of the Royal Society* as a mathematical demonstration of the Copernican system. Certainly the atmosphere was embarrassing for the 'free philosophising about the things of the world and nature' which Galileo had pleaded so earnestly to keep open. Richelieu instigated an attempt to have the Copernican theses condemned at the Sorbonne, but without success: it was decided that the question was one for philosophy, not for authority. It was

on hearing of Galileo's condemnation that Descartes, already a nervous philosopher and living in Holland, explicitly adopted his policy of dissembling in philosophy and became, in Maxime Leroy's phrase, the *'philosophe au masque.'* In November 1633 he wrote in alarm to Mersenne, who was arranging the publication of *Le Monde*, asking for news of the affair of the Copernican theory: 'and I confess that if it is false, then so are the whole foundations of my philosophy, because it is demonstrated from them beyond doubt.' When he discovered what had happened he sent Mersenne further letters withdrawing *Le Monde*, writing in April 1634:

> No doubt you know that Galileo was arrested a short time ago by the Inquisitors of the Faith, and that his opinion concerning the movement of the Earth has been condemned as heretical. Now I would like to point out to you that all the things that I explained in my Treatise, among which was this opinion about the movement of the Earth, depend so much upon the other that it is enough to know that one of them is false, to know that all the reasons which I used are invalid; and although I thought that they were based on very certain and evident demonstrations, I would not wish for anything in the world to maintain them against the authority of the Church. I know well that it could be said that everything that the Inquisitors of Rome have decided does not thereby become an incontinent article of faith, and that it would first be necessary for it to be accepted by the Council. But I am not so much in love with my thoughts as to want to make use of such qualifications in order to maintain them; and I want to be able to live in peace and to continue the life I have begun in taking as my motto: *bene vixit, bene qui latuit* [he lives well who keeps well out of sight], accepting the tact that I am happier to be delivered from the fear that I would make more acquaintances than I want through my book than sorry to have wasted the time and trouble I have spent in writing it. . . . I have seen a notice about the condemnation of Galileo, printed at Liége on the 20th

of September 1633, in which these words occur: *quamvis hypothetice a se illam proponi simularet* [he pretended to present hypothetically what he was proposing], so that they even seem to forbid the use of this hypothesis in astronomy; . . . having seen nowhere that this censure has been authorised by the Pope or by the Council, but only by a particular Congregation of Cardinal Inquisitors, I do not lose all hope that the same thing will happen with it as with the Antipodes, which were more or less condemned at one time, and so that my *Monde* will be able to see the light of day in the course of time; towards which circumstance I will have to use my wits.

When Descartes eventually published his cosmology in the *Principa Philosophiæ* in 1644, it was under the cover of his presentation of physical theories as fictions (cf. below, p. 319). 'I want what I have written to be taken simply as an hypothesis,' he wrote, 'which is perhaps far removed from the truth.' With the definition that he had developed of motion as *simply* translation from proximity to one set of bodies to proximity to another set, he was able to suppose that all motion was completely relative, any set of bodies being able to be chosen arbitrarily as the point of reference at rest. This enabled him to declare formally that the earth could be considered at rest. The conventionalism and fictionalism forced on physicists by the anti-Copernican decree had entered deeply into Descartes' soul, and earned him the polemics of Newton. The decree and the theological ambience in which it was issued had a greater responsibility for the more 'positivist' aspects of 17th-century thought than may sometimes be realised (cf. below, p. 314 *et seq.*).

Descartes had seen the important point that without papal ratification the Copernican theses had not been formally declared contrary to faith and heretical. The same point was made by Gassendi. The Commissioner General himself, Vincenzo Maculano da Firenzuola, who had conducted the proceedings against Galileo, admitted to Galileo's Benedictine pupil and friend Benedetto Castelli that astronomical questions could not be decided by Scripture,

which was concerned only with matters relating to salvation. In the decades that followed a number of Jesuit writers made the same point as Descartes and Gassendi. For example the French Jesuit Honoré Fabri, writing in 1661 in defence of the geocentric passages of Scripture, added that if conclusive reasons were ever found he did not doubt that the Church would say that they should be understood 'figuratively.' It was not until 1757 that Pope Benedict XIV annulled the anti-Copernican decree. At length in 1893 Pope Leo XIII made the *amende honorable* to Galileo's memory by basing his encyclical *Providentissimus Deus* on the principles of exegesis that Galileo had expounded, and rejected the fundamentalism of Bellarmine and of the Qualifiers of the Holy Office.

Not to be outdone, Pierre Duhem in 1908 made his famous declaration, in his *Essai sur la notion de théorie physique de Platon à Galilée* (*Annales de philosophie chrétienne*, 1908, vol 6, pp. 584-85, 588), that more recent developments in physics had shown that 'logic was on the side of Osiander, Bellarmine and Urban VIII and not on that of Kepler and Galileo, that the former had grasped the exact significance of the experimental method, while the latter had been mistaken.' . . . 'Suppose the hypotheses of Copernicus were able to explain all known appearances, what can be concluded is that they may be true, not that they are necessarily true, for in order to legitimate this last conclusion, it would have to be proved that no other system of hypotheses could possibly be imagined which could explain the appearances just as well; and this last proof has never been given.'

Duhem was making the valid point, fully developed in his *La Théorie Physique: son objet, sa structure* (1914), that experiment can never *establish* a theory irrefutably. But by introducing the dynamical criterion for choosing between two theories equally accurate in 'saving the appearances' of the heavens Galileo was in effect introducing a test of a theory by its range of applicability, as indeed Duhem realised. By this test it may be claimed that Galileo and Kepler showed how to go about *refuting* an astronomical theory and that in fact Newton did refute the

geocentric hypothesis.[16] In this way the experimental falsi-
fication of the consequent could make it necessary to *deny*
the antecedent, even though its verification would not en-
able the antecedent to be affirmed. Leaving aside Duhem's
misapprehension of Bellarmine's very restricted application
only to astronomical theories of the 'positivist' interpreta-
tion of science advocated by Duhem himself, the view that
the two rival theories were merely alternative calculating
devices certainly does not survive Galileo's test.

Discussing this controversy in 1844, J. H. Newman, the
future cardinal, wrote in his *Sermons chiefly on the Theory
of Religious Belief*: 'If our sense of motion be but an ac-
cidental result of our present senses, neither proposition
is true and both are true, neither true philosophically, both
true for certain practical purposes in the system in which
they are respectively found.' Newman was not of course
trying to make a revolution in logic but to deal with a
difficulty in a theological controversy, but a similar point
is sometimes made by those who say that Einstein's prin-
ciple of general relativity has made Galileo's problem
meaningless because motion and rest can be defined only
by reference to a conventional standard, so that it is equally
legitimate to take a stationary earth or a stationary sun for
the frame of reference. But for general relativity it makes
just as much sense to say that the earth rotates, as it did
for Galileo and for Newton. To take a medieval example,
it can be said to rotate just as a millstone can be said to
rotate: it rotates with reference to all local inertial systems.
It was in this sense that the earth's motion was at issue.
A sophisticated logical interpretation of science is unavoid-
ably faced with the fact that theoretical scientific analysis
can make genuine physical discoveries, even though Gali-
leo's assertion that a theory empirically verified according
to his principles is a 'necessary' truth must be regarded
as evidence that he himself was still the prisoner of a too
simple Euclidean model for physics.

(3) PHYSIOLOGY AND THE METHOD OF EXPERIMENT AND MEASUREMENT

Experimental physiology was another branch of science in which the quantitative approach, which Galileo had used with such success in mechanics and which was to achieve such astonishing triumphs in astronomy, was used with great effect in the 17th century.

Galileo himself had shown, while studying the strength and cohesion of materials, that whereas weight increased as the cube, the area of cross-section, on which strength depended, increased only as the square of the linear dimensions. There was thus a definite limit to the size of a land animal which its limbs could bear and its muscles move, but animals living in water, which supported the weight, might reach enormous dimensions.

One of the first to apply Galileo's methods to physiological problems was his colleague, the professor of medicine at Padua, Santorio Santorio (1561–1636). He described a number of instruments such as a pulsilogium, or small pendulum for measuring pulse-rate, and a clinical thermometer. He used the latter to estimate the heat of a patient's heart by measuring the heat of the expired air, which was supposed to come from the heart. He also made instruments to measure temperature in the mouth and others to be held in the hand. His method of measurement was to observe the distance which the liquid in the thermometer fell during 10 beats of a pulsilogium. As this depends not only on the patient's temperature but on the speed of his peripheral circulation, which increases with fever, Santorio's measurement of the rate of rise of temperature was probably an excellent indication of fever. In another work, *De Medicina Statica* (1614), he described an experiment which laid the foundation of the modern study of metabolism. He spent days on an enormous balance, weighing food and excrement, and estimated that the body lost weight through 'invisible perspiration.'

It was to William Harvey (1578–1657) that the revolu-

tion in physiology was chiefly due. After graduating at Cambridge, Harvey spent five years at Padua under Hieronymo Fabrizio of Aquapendente (1537–1619), who was Galileo's colleague and personal physician. There Harvey learnt from his revered teacher to value the comparative method (see below, pp. 280–81). Most of his own researches into comparative anatomy were lost during the English Civil War, but in the two books which contain his extant contribution to science he emphasised the importance of comparative anatomy, both for its own sake and for elucidating the structure and physiology of man. He examined the hearts of a large number of vertebrates, including lizards, frogs and fish, and of invertebrates such as snails, a small transparent shrimp and insects. In insects he observed the pulsating dorsal vessel with a magnifying glass. Although his period at Padua coincided with Galileo's professorship there, there is no evidence that they ever met, nor did Harvey ever mention Galileo in his works. Nevertheless Harvey's method of restricting his research into biological processes to problems which could be solved by experiment and measurement might well have been learnt from the great mechanist. At any rate he breathed the same atmosphere and, although his references to logic were almost entirely to Aristotle, he also resembles Galileo in that his most important work was a perfect practical exhibition of the methods of 'resolution' and 'composition.'

Harvey's notes for lectures given at the Royal College of Physicians in London in 1615–16, and published as the *Prelectiones Anatomiæ Universalis* in 1886, show that he had then already arrived, through experiment and reflection, at the idea of the general circulation of the blood. Several of the constituents of this idea had already been discovered by his predecessors, but no one before him had seen that the difficulties raised by Galen's account of the motion of the blood were such as to require a revision of the whole theory. In fact Harvey's originality, no less than Galileo's, sprang from his ability to see familiar facts from an entirely novel point of view. The essential anatomy of the vascular system had been known since Galen's time and was as familiar to Harvey's immediate predecessors as to

himself. It was not on purely anatomical grounds that he was able to reject Galen's complete separation between the venous and arterial systems (cf. Vol. I, p. 164 *et seq.*). He made his reinterpretation on the basis of a total shift in physiological theory; once this was accepted, the anatomical structures all fell into place in the new scheme.

The chief points in Galen's theory that became problems for Harvey were his assertions (i) that venous blood was produced continuously in the liver from the food, (ii) that it flowed in the veins out from the liver to all parts of the body, (iii) that only a small fraction of it entered the heart itself, and found its way from the right to the left ventricle to be converted into the arterial blood (this raised the questions of the existence of the pores in the interventricular septum and of the pulmonary circulation), (iv) his assertion that the arterial blood was drawn out of the heart in diastole and his account of the arterial pulse, and (v) his account of the two-way motion of air and waste in the venous artery. The first point raised the question of the amount and speed of the blood travelling in the vessels, and the others those of the direction of flow and of the action of the heart. None of those was considered except in isolation by any of Harvey's predecessors.

Leonardo da Vinci had maintained that the heart was a muscle, and made admirable drawings of it which included the discovery of the moderator band in the sheep. He had also followed the movements of the heart in the pig by means of needles thrust through the chest wall into its substance, and constructed models to illustrate the action of the valves. His views on the movements of the blood were, however, almost entirely Galenic and, moreover, it is not known whether his anatomical manuscripts had an influence similar to those on mechanics (Pl. XII). The French physician, Jean Fernel, seems to have been the first to have observed, in 1542, that, in contradiction to current teaching, when the ventricles contracted (systole) the arteries *increased* in size, and to have stated that this was because of the blood (and compressed spirits) entering them. But in general Fernel expressed the ideas accepted before Harvey, relating the motion of the heart primarily to the sup-

posed cooling function of respiration, and arguing in
favour of Galen against Aristotle on the cause of its action
and of the pulse. In 1543, Vesalius published his observa-
tions showing that he had been able to discover no pores
through the interventricular septum; he had probed the
pits in the septum, and found that 'none of these pits pene-
trate (at least according to sense) from the right ventricle
to the left' (cf. Pl. XI). In the second edition (1555) of
his *De Fabrica* (see below, pp. 274–75) he was even more
definite about the absence of pits, remarking: 'I am not
a little hesitant concerning the heart's function in this
respect.'[17]

A similar doubt, together with the view that the heart
was a muscle and had two and not three ventricles, had
already been asserted by the 13th-century Egyptian (or
Syrian) physician, Ibn al-Nafis al-Qurashi. Ibn al-Nafis had
maintained, as against Avicenna and Galen, that, since
there was no passage through the septum, venous blood
must pass from right to left ventricle via the arterial vein
(pulmonary artery), through the lungs, where it spread
through their substance and mixed with the air in them,
and then back to left side of the heart in the venous artery
(pulmonary vein). This work seems to have been unknown

---

[17] It was the accepted interpretation in the 16th century that
Galen had held that the blood passed from the right to the left
side of the heart through these pores. This was also the view of
Avicenna (cf. *Canon medicinæ*, III. xi. i. 1, Venice, 1608, i 669–
70), although Galen's own writings seem to leave open the pos-
sibility that some blood passed through the lungs (see above,
Vol. I, p. 165). Harvey himself may have interpreted Galen in
the latter sense, although his remarks are equivocal: 'From Galen,
that great Prince of Physicians, it seems clear that the blood
passes through the lungs from the arterial vein [pulmonary ar-
tery] into the minute branches of the venous artery [pulmonary
vein], urged to this both by the beating of the heart and by the
movements of the lungs and thorax.' (*De Motu Cordis*, Ch. 7)
At least Harvey did pay Galen the tribute of having provided
clear evidence for the pulmonary circulation by his description of
the cardiac valves and of the anastomosis of arteries and veins in
the lungs; but he ridiculed the view that a current of 'sooty wastes'
could flow back through the mitral valves from the left ventricle
to the lungs.

in the West[18]; the first Western writer to publish the theory of the pulmonary circulation (1553) was the Catalan scholar Miguel Serveto (1511–53), who mentioned, in the course of a theological discussion, that some blood passed from right to left ventricle via the lungs, where it changed colour. He supposed some also to pass through the interventricular septum. Serveto's interests were primarily theological and it is probable that he derived these ideas from some other source, although he had in fact studied anatomy, having been a pupil of Johannes Günther of Andernach in Paris at the same time as Vesalius himself. There is at present no evidence that either he or the Paduan anatomist Realdo Colombo (c. 1516–59) knew of Ibn al-Nafis, and some scholars have suggested that it was Serveto who inspired Colombo in his views on the lesser circulation. In view of the curious context in which Serveto announced the discovery, others have suggested that the borrowing was more probably the other way round; it is even possible that Colombo derived the idea of the lesser circulation from Vesalius himself, whose pupil he had been at Padua. Colombo himself, in his *De Re Anatomica* (1559), not only put forward the idea of the pulmonary circulation but also supported it with experiments. He noted, as Fernel had done, that cardiac systole (contraction) coincided with arterial *expansion* and cardiac diastole (expansion) with arterial contraction; and he showed, further, that the complete closure of the mitral valve prevented pulsation in the pulmonary vein. When he opened this vein he found not fumes, as the Galenists would have expected, but blood, and he concluded that blood passed from the lung (where a change in colour was observed) through the pulmonary vein back to the left side of the heart. Like Serveto, he believed that some blood passed also through the interventricular septum. Both writers held also to the Galenic view that blood was made in the liver. Thus neither had any idea of the true nature of blood, and although Colombo

---

[18] A Latin translation by Andrea Alpago of Ibn al-Nafis' great commentary on Avicenna's *Canon* was published in Venice in 1547, but, curiously enough, this omitted the section on the pulmonary circulation.

had observed that the pulsation of the brain was synchronous with that of the arteries, he did not arrive at the idea of the general or systemic circulation.

The same may be said for Colombo's Catalan pupil, Juan Valverde, who gave an account of the lesser circulation in 1554. Valverde seems to have claimed no originality for himself, and some scholars have argued, on the grounds that he stated, like Serveto, that the pulmonary vein contains both blood and air, that it was Serveto who influenced him. Against this others have argued that it was from Colombo's teaching that Valverde learnt of the idea of the lesser circulation; Colombo's treatise, published posthumously in 1559, may well have been written before Valverde's. Certainly Colombo himself claimed the new idea as his own, and hitherto unknown.

The Dutch anatomist Volcher Coiter (1534–c. 1576) also made some experiments on the heart. He made a comparative study of the living hearts of kittens, chickens, vipers, lizards, frogs and eels, and observed that in the excised organ the auricles contracted before the ventricles and that the heart was lengthened in systole and shortened in diastole. He also showed that a small detached piece of heart muscle would continue to beat.

Some observations on the movements of the blood were made also by the Italian physiologist and botanist, Andrea Cesalpino (1519–1603). He said, in his *Quæstionum Peripateticarum* (1571), that when the heart contracted it forced blood into the aorta and when it expanded it received blood from the *vena cava*. In his *Quæstionum Medicanum* (1593), book 2, question 17, he said:

> The passages of the heart are so arranged by nature that from the *vena cava* a flow takes place into the right ventricle, whence the way is open into the lung. From the lung, moreover, there is another entrance into the left ventricle of the heart, from which there is a way open into the aorta artery, certain membranes being so placed at the mouths of the vessels that they prevent return. Thus there is a sort of perpetual movement from the *vena cava* through the heart and lungs into the aorta

artery, as I have explained in my *Peripatetic Questions*. If we take into account that in the waking state there is a movement of natural heat towards the exterior, that is to say, towards the organs of sense, while in the sleeping state there is, on the contrary, a movement towards the interior, that is, towards the heart, we must judge that in the waking state much of the spirit and blood become engaged in the arteries, since it is by them that access is had to the nerves, while, on the other hand, in sleep the animal heat comes back through the veins to the heart, but not by the arteries, since the access provided by nature to the heart is through the vena cava and not through the aorta . . . For in sleep the native heat passes from the arteries into the veins through the process of communication called anastomosis, and thence to the heart.

He used this to explain the observation that when a vein was ligatured it swelled up on the side away from the heart. But his views on the subject lacked clarity and decision, and in his last work in 1602–3 he formally stated that blood went *forth* from the heart through the veins as well as the arteries. Though he used the word *circulatio*, he understood this to mean a to-and-fro movement as in the rising and falling of fluid, evaporation followed by condensation, in chemical distillation. Thus he did not understand the general circulation any more than Colombo, Serveto or Valverde, or Carlo Ruini who, in 1598, also published a description of the pulmonary or lesser circulation in his treatise on the anatomy of the horse, or Fabrizio who, in 1603, gave the first clear and adequate figures of the valves in the veins but believed that their function was to counteract the effect of gravity and prevent the blood accumulating in the hands and feet. (These valves had been described by Charles Estienne in 1545—see below, p. 272, and after that they were studied by several anatomists, none of whom understood their action.)

The theory of the general circulation of the blood was, in fact, first advanced by William Harvey and published in 1628 in his *Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus*. The earliest knowledge of his

great theory comes, however, from the *Prelectiones*, and these provide valuable evidence of how he reached it.

There is a conversation recorded by Robert Boyle in 1688 but relating to over thirty years earlier, although still nearly twenty years after the publication of *De Motu Cordis*, in which Harvey himself seems to connect his theory with the results of the great Italian tradition of anatomical studies. 'I remember,' Boyle wrote, 'that when I asked our famous Harvey, in the only discourse I had with him (which was but a while before he died), what were the things that induced him to think of the circulation of the blood, he answered me that when he took notice that the valves in the veins of so many parts of the body were so placed, that they gave free passage to the blood towards the heart, but opposed the passage of the venal blood the contrary way, he was invited to imagine that so provident a cause as nature had not so placed so many valves without design; and no design seemed more probable than that since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries, and return through the veins, whose valves did not oppose its course that way.' (Boyle, *Works*, 1772, vol. 5, p. 427)

More recently it has been suggested that Harvey's theory of the general circulation was a natural development of the work of his predecessors on the pulmonary circulation. Neither of these suggestions receives much support from his own writings, but at another level, that of method, the Italian tradition is clear indeed. Harvey himself shows us that his great illumination came from his use of the comparative method; his ability to follow out its consequences came from his clear grasp of the use of experiment and of measurement. All this was the teaching of Padua, but it was the use to which he put those methods that raised him to an altogether higher level of originality.

This can be seen in the contrast between him and the anatomists who had discussed the pulmonary circulation. These had never questioned the basic Galenic assumption that the veins and the right side of the heart formed a system, centred on the liver, which was quite distinct in

function and structure from the system formed by the arteries and the left side of the heart. Between the two lay the lungs, receiving nutriment from the venous blood sent from the right ventricle, providing from the air the principle of its conversion into arterial blood in the left ventricle, and cooling and cleansing the heart itself. They had looked for the solution of one particular problem: how the blood passes from the right to the left side of the heart in man, a problem that arose and was solved within Galen's system itself. Looking beyond man to a whole range of red-blooded animals and even to animals like shrimps, insects and snails, Harvey saw that this was only part of the more general problem of the movement of the blood in the body as a whole. In fishes, which have no lungs, in frogs, toads, snakes and lizards which resemble fish in having only a single ventricle, and also in the embryos of lunged animals, the first problem did not in fact arise at all. 'The common practice of anatomists,' he wrote in chapter 6 of *De Motu Cordis*, 'in dogmatizing on the general make-up of the animal body, from the dissection of dead human subjects alone, is objectionable. It is like devising a general system of politics, from the study of a single state, or pretending to know all agriculture from an examination of a single field. It is fallacious to attempt to draw general conclusions from one particular proposition.' 'Had anatomists only been as conversant with the dissection of the lower animals as they are with that of the human body, the matters that have hitherto kept them in a perplexity of doubt would, in my opinion, have met them freed from every kind of difficulty.'

Far from being simply a continuation of the work of his predecessors, the main object of Harvey's whole argument was to put forward, and to demonstrate by experiment and accessory evidence, a conclusion diametrically opposed to their basic Galenic assumptions about the course of the blood and the action of the heart. The question of the pulmonary circulation plays a very secondary role in his whole argument; indeed he discussed it fully only in a letter written in 1651, to Paul Marquard Slegel of Hamburg. Harvey's originality was altogether greater than the sum of

the contributions of his predecessors. What he did was to be the first, since Galen, to attempt 'a general system of politics' in questions of anatomy and physiology. He was the first to produce a theory which, as he insisted both in *De Motu Cordis* and in the controversies to which it gave rise, comprehended all the particular circulatory systems of different animals and embryos in a general scheme. By demonstrating an alternative to the central doctrine of Galen's system, he raised an entirely new range of questions about physiology in general.

Harvey's discussion, both in the *Prelectiones* and in *De Motu Cordis*, indicates that it was Galen's assertion that the blood left the heart in diastole and his account of the arterial pulse that led to his first doubts. The argument in the *Prelectiones* closely follows that in the first eight chapters of *De Motu Cordis*. Both begin with a 'resolution' of the problem into its parts, so that the cause might be discovered through its effects. After analysing the difficulties for Galen's theory, citing many observations made by others, he concentrated on demonstrating the action of the heart in systole, the nature of the pulse, and the consequent continuous flow of the blood through the heart, in various animals and in the foetus, as a result of its continuous beat. The *Prelectiones* conclude with a statement of the hypothesis of the general circulation similar to that in chapter 8 of *De Motu Cordis*. Probably the discussion in his lectures stopped there, for he was demonstrating the anatomy of the thorax as a whole, and this had to be completed in one day because there were no preservatives. The remaining chapters of *De Motu Cordis* clearly form a second section corresponding to the 'compositive' part of the argument. He described the testing of his hypothesis by three consequences that follow from it; stated it definitely in chapter 14; and added further accessory evidence.

He began his demonstration by showing that the contraction of the heart was a muscular contraction beginning with the auricles and passing to the ventricles, whose contraction then caused the expansion of the arteries. In contradiction to the conceptions of its action held by Aristotle and by Galen, he concluded that the heart was in fact a

force pump. This suggested that there was a flow of blood from the veins through the heart into the arteries, and the arrangement of the cardiac valves would prevent its return. He then showed that if either the pulmonary artery or the aorta alone were punctured, the contraction of the right ventricle was followed by a jet of blood from the former, and the contraction of the left ventricle by a jet of blood from the latter; the two ventricles contracted and dilated in unison. In the fœtus he pointed out that the structure of the heart and vessels was designed to bypass the lungs, which were not yet functioning. He said that the blood from the *vena cava* passed through an opening, the *foramen ovale*, into the pulmonary vein and so via the left ventricle into the aorta. (Actually the *foramen ovale* opens directly into the left auricle.) The blood entering the pulmonary artery was carried into the aorta by the fœtal *ductus arteriosus.* The two ventricles thus operated as one, and the condition in the embryo of animals with lungs corresponded to that in the adults of animals such as fish which had no lungs. In the adults of the animals with lungs the blood could not pass through the two fœtal passages, which were closed, but had to go from the right to the left side of the heart via the tissue of the lungs themselves.

From the structure and continuous beat of the heart Harvey concluded that the flow of the blood through it was not only in one direction but also continuous. It would follow from this that unless there were some passage from the arteries back to the veins in the body at large, as well as in the lungs, the veins would soon be drained and the arteries would be ruptured from the quantity of blood flowing into them. There was, therefore, no escape from the hypothesis which he enunciated in chapter 8 of *De Motu Cordis:*

> I began to think whether there might not be *a motion, as it were, in a circle.* Now this I afterwards found to be true; and I finally saw that the blood was forced out of the heart and driven by the beating of the left ventricle through the arteries into the body at large and into its

several parts, in the same way as it is sent by the beating of the right ventricle through the arterial vein [pulmonary artery] into the lungs, and that it returns through the veins into the *vena cava* and so to the right ventricle, in the same way as it returns from the lungs through the venous artery [pulmonary vein] to the left ventricle.

Proceeding to the testing of this hypothesis, Harvey next made a number of deductions which, if experimentally verified, would both confirm it and finally eliminate the rival hypothesis of Galen that the blood was continuously produced in the liver from the ingested food. First, he demonstrated that, with the blood flowing continuously in one direction through the heart, it could be calculated from the heart's capacity and rate of beat that it pumped through itself in an hour, from the veins to the arteries, more than the whole weight of the body. That the blood did flow continuously through the heart only in the direction from veins to arteries, he confirmed by further experiments. In a serpent, whose vessels were conveniently arranged for experimental investigation, when the *vena cava* was pinched with forceps the heart drained and became pale, whereas when the aorta was similarly closed the heart became distended and purple. This was in keeping with the arrangement of the valves. Secondly, he showed, by experiments with ligatures, that this same large amount of blood that passed through the heart was forced through the arteries to the peripheræ of the body, and that there the blood flowed in the same continuous stream in one direction only, but, in those regions, from arteries to veins. In the limbs the arteries are deeply placed, while the veins are near the surface. A moderately tight ligature round the arm would constrict the latter but not the former, and he found that this produced a distension of the hand with blood. A very tight ligature stopped the pulse and flow of blood into the hand altogether and no distension was observed. Finally, he showed that the blood returned to the heart in the veins. Anatomical investigations showed that the valves were arranged in the veins so that the blood could flow only towards the heart, a fact which Fabrizio

had not realised. Harvey showed that when the arm was
ligatured moderately tightly so that the veins swelled up,
'nodes' were formed at the position of the valves (Pl.
XIII). If the blood were pushed out of the vein below the
valve by running the finger along it in the peripheral direc-
tion, the emptied section remained flat, and he concluded
that this was because the valve prevented the blood from
running back into it. This explanation he confirmed by fur-
ther experiments of the same kind. He therefore arrived
at the definitive conclusion, in *De Motu Cordis*, chapter
14:

> Since all things, both argument and ocular demonstra-
> tion, show that the blood passes through the lungs and
> heart by the action of the ventricles, and is sent for dis-
> tribution to all parts of the body, where it makes its way
> through the pores of the flesh into the veins, and then
> flows by the veins from the circumference on every side
> to the centre, from the lesser to the greater veins, and is
> by them finally discharged into the *vena cava* and right
> auricle of the heart, and this in such a quantity, with
> such an outflow through the arteries, and such a reflux
> through the veins, as cannot possibly be supplied by the
> ingesta, and is much greater than can be required for
> mere purposes of nutrition; it is therefore necessary to
> conclude that the blood in the animal body is impelled
> in a circle, and is in a state of ceaseless motion; that this
> is the act or function which the heart performs by
> means of its pulse; and that it is the sole and only end
> of the motion and contraction of the heart.

Published at Frankfurt, the scene of an annual book fair,
Harvey's treatise became widely distributed. In spite of
criticism by some established professors such as Jean
Riolan of Paris, his theory was fairly soon adopted, espe-
cially by younger anatomists, an example of the fact that
often only a new generation can appreciate a fundamental
revolution, partly because to it the new doctrine has ceased
to be revolutionary. John Aubrey wrote in his vignette of
Harvey: 'I have heard him say, that after his booke of the
Circulation of the Blood came out, that he fell mightily in

his practize, and that 'twas beleeved by the vulgar that he was crack-brained; and all the physitians were against his opinion, and envyed him; many wrote against him, as Dr. Primige, Paracisanus, etc. With much adoe at last, in about 20 or 30 years time, it was received in all the Universities in the world; and, as Mr. Hobbes sayes in his book 'De Corpore,' *he is the only man, perhaps, that ever lived to see his owne doctrine established in his life time.*'

Harvey's theory was an immense illumination to physiology, to which it directed the interest of all biologists. His treatise provided a model of method. After him, abstract discussion of such questions as the nature of life or of 'innate heat' gradually gave way to the empirical investigation of how the body worked. He himself had left somewhat vague the passage of the blood from arteries to veins, and the demonstration of his theory was finally completed when, in 1661, Malpighi observed, under the microscope, the flow of blood through the capillaries in the frog's lung. About the same time Jean Pecquet and Thomas Bartholin worked out the lymphatic system, beginning with Pecquet's observations, at the end of Harvey's life, on the lacteals, the vessels that carry the chyle (emulsified fat) from the small intestines to the veins by way of the thoracic duct— an important complement to Harvey's theory which the aged physiologist himself rejected on the very grounds of comparative anatomy that had guided his own work. He could find no trace of lacteals in birds and fish. 'Nor,' he wrote to Dr. R. Morrison, 'do I see any reason why the route by which the chyle is carried in one animal should not be that by which it is carried in all animals whatsoever; nor indeed, if a circulation of the blood be necessary in this matter, as it really is, that there is any need for inventing another way.' Those very aptitudes for theoretical generalisation to which he owed his greatest discoveries were to blind him to the apparent inconsequentiality of fact.

The study of the blood, the bearer of food and oxygen, was, in fact, well placed to lay the foundations of physiology, and Harvey's elucidation of its mechanics was followed later in the 17th century by the researches especially of Boyle, Hooke, Lower and Mayow into the chemical prob-

lem of respiration, which they related for the first time to the general problem of combustion.

But Harvey himself never understood the function of respiration, and when we come to consider his views on the purpose of the circulation in general we must put ourselves into the framework of a philosophy of nature very different from that of modern physiology, a framework of questions extending beyond the range of those to which Harvey's elucidation of the mechanics of the circulation was the positive answer incorporated into modern science.

The philosophy of nature of a period different from our own, the whole complex of presuppositions and conceptions that a particular explanation eminently satisfies at a given time, is often more clearly indicated by secondary writers than by the great innovators whose originality inevitably transforms the background of ideas into which they were born. One of the first of contemporaries to accept Harvey's theory was the London physician, alchemist and Rosicrucian Robert Fludd, many of whose own writings had been issued by the same Frankfurt publisher. But Fludd saw in the great discovery of 'his friend, colleague and compatriot, well versed not only in anatomy but also in the deepest mysteries of philosophy,' as he called Harvey in his *Integrum Morborum Mysterium* in 1631, not the beginning of a new physiology, but a demonstration of something quite different: of the correspondence between the microcosm of the body and the macrocosm of the celestial spheres; a demonstration that the spirit of life retained an impression of the planetary system and of the zodiac, an impression of the circular motion of the heavenly bodies that ruled the world below.

Cool, clear and rational as he was, an empirical scientist to the depths of his mind, it is clear that Harvey himself would not have been unwilling to accept this compliment. At the end of the passage already quoted from chapter 8 of *De Motu Cordis*, in which he described how the idea of circulation came into his mind, Harvey related the motion of the blood to a general view of the world. A true pupil of Padua, his view is basically Aristotelian· 'The authority of Aristotle has always such weight with me that I never think

of differing from him inconsiderately,' as he said later in *De Generatione Animalium* (exercitatio 11). It was basic to Aristotle's philosophy of nature that circular motion is the noblest form of motion and that the circular motion of the heavenly bodies forms the pattern to which the motions of sublunary bodies, and especially of the microcosm of living organisms, aspire. Aristotle had made the heart the principle organ of the body and the origin of the blood and vessels. After his account of the mechanical pumping of the blood round the body by the heart, Harvey likens its circular motion to the cycle of water evaporating under the sun's heat from the moist earth and returning again as rain, thus producing the generations of living things, and to the annual cycle of the weather with the approach and retreat of the sun: both 'as Aristotle says . . . emulate the circular motion of the superior bodies.'

> And so in all likelihood does it come to pass in the body, through the motion of the blood. All the parts may be fed, warmed and quickened by the warmer and more perfect vaporous, spirituous and, as I may say, nutritive blood; and this, on the contrary, may become, in contact with the parts, cooled, thickened, and so to speak effete, so that it returns to its origin, the heart, as to its source, the inmost temple of the body, to recover its perfection and virtue. Here it is again liquified by natural heat—potent, burning, a kind of treasury of life, and it is impregnated with spirits and as it might be said with balsam; and thence again it is dispersed; and all this depends on the motion and beating of the heart. Consequently the heart is the beginning of life, the sun of the microcosm, just as the sun in his turn might well be called the heart of the world; for it is by the potency (*virtus*) and beating of the heart that the blood is moved, perfected and quickened to life (*vegetatur*) . . . for the heart indeed is the perfection of life, the source of all action.

This view of the cosmological pattern in which the circulation of the blood took its place, Harvey shared with another Aristotelian, Cesalpino. Like Harvey, Cesalpino

had regarded the renewed 'perfection' of the blood as the immediate purpose of its passage through the heart; and like Harvey he described a cyclical process of heating and evaporation in the heart, followed by cooling and condensation in the parts of the body, corresponding to the alchemical cycle of *distillatio*. These notions, the analogy of the microcosm and the macrocosm, the prevalence of cycles in nature, the excellence of the circle, were in fact commonplaces and occur in various forms in all the Aristotelian, alchemical, Paracelsist, and Neoplatonic writings of the period. They appear, for example, in the symbolical embryology of Peter Severinus (1571) and of Johann Marcus Marci of Kronland (1635). Harvey himself returned to them in his *De Generatione Animalium* (1651) as the analogy of the coming and going of new generations, especially in the cycle of change, described in his theory of 'epigenesis,' from the undifferentiated seed to the first differentiated matter, the blood, thence to the fully differentiated adult, and back to the seed forming the new generation.

It is this philosophical conception of cycles that united the two great fields of Harvey's work (see below, pp. 282–83), and this is a good illustration of the fact that if we are to understand the appearance of a discovery or a new explanation, and the particular form it takes, we must look beyond the purely empirical grounds on which it rests. The latter indeed are never alone in determining the scientist's expectations and the direction of his attention and his vision; these are inevitably to some extent the products of a theory, and certainly in Harvey's case of unverified ontological assumptions about the world which constituted his philosophy of nature. But the difference between a scientist like Harvey and the mere speculators like Fludd, with whom he may have shared many such assumptions, was that he put his theories to effective empirical tests. In this he stood in the same relation to Fludd as did Kepler. To the end of his life Harvey denied that the blood underwent any essential change in the lungs; he held that the blood was cooled in the body generally and he thought that the traditional view might be correct, that breathing cooled it

especially. But he distinguished this problem from the *fact* of the circulation: 'I own I am of opinion,' he wrote in the *Second Disquisition to Jean Riolan* (1649), 'that our first duty is to inquire whether the thing be or not, before asking wherefore it is.' Harvey's great strength as a master of the experimental method, and his superiority over all other contemporary biologists, was that he had both the gifts of imagination that made him a great discoverer and a great theoretician, and the gifts of reason that showed him how to test his theories by precise and quantitative experiments.

It was the theoretical gifts that were uppermost in the mind of the co-founder, with Harvey, of modern physiology, Descartes. In his *Discours de la Méthode* (1637) Descartes had expressed the hope of arriving at rules which would reform medicine in the same way as he had attempted to reform the other sciences. He was one of the first to accept Harvey's discovery of the circulation of the blood, though he did not understand the pumping action of the heart, which he still regarded as producing its action through its vital heat. Although he gave the credit for the discovery of the circulation to '*un médécin d'Angleterre*' (*Discours*, part 5) Descartes claimed for himself the elucidation of the mechanism of the heart. He thought that it was the vital heat of the heart that caused it to expand by vapourising the blood drawn into it on contraction, and that it was this expansion in diastole that sent the blood along the arteries and into the body and the lungs, where it cooled and liquified and returned to the heart, where the cycle began again. Descartes was in fact reviving Aristotle's explanation, in opposition to both Galen and Harvey (cf. Vol. I, p. 167; below, p. 309 *et seq.*). It is indeed curious that a man who claimed to have divested himself of all former prejudices should have repeated the old error, already detected a century before, that the blood left the heart in diastole, and that his physiological system as a whole should so much have resembled those of Galen and Aristotle. But it is not by such details that Descartes' achievement should be judged; indeed had they caused him any hesitation perhaps he would never have made it. His contribution was to grasp and assert one big theoretical

idea: that the body is a machine, and that all its operations are to be explained by the same physical principles and laws as apply in the inanimate world. Though he still used terms like 'spirits,' these were simply material, and they obeyed general mechanical laws; the special spirits and principles charged in the old physiology with each particular function had been eliminated. Whereas the philosophy of nature, the system of analogies with cycles of nature and with the sun, within which Harvey worked out how the heart and blood moved was of little use in suggesting further inquiries, Descartes' mechanism was immediately fruitful. In spite of his misunderstanding, he had made a point against Harvey in pressing the question of the *cause* of the heart's beating. He wanted to show that this would follow from known mechanical laws, and so appear as a phenomenon expected within the general system of mechanics.

'But lest those who are ignorant of the force of mathematical demonstrations,' he wrote in part 5 of the *Discours*, 'and who are not accustomed to distinguish true reasons from mere verisimilitudes, should venture, without examination, to deny what has been said, I wish it to be considered that the motion which ⹂ have now explained follows as necessarily from the very arrangements of the parts, which may be observed in the heart by the eye alone, and from the heat which may be felt with the fingers, and from the nature of the blood as learned from experience, as does the motion of a clock from the power, the position and the shape of its counterweights and wheels.'

In his presentation of his mechanistic theory Descartes made explicit an even greater contribution to physiology, for he did so in terms of one of the most fruitful *methods* known to science: the method of the theoretical model. A theoretician of scientific method as well as of physics and physiology, Descartes was perfectly conscious of what he was doing; it was he who made the method of the physical and chemical model the powerful tool of analysis it has ever since been in physiological research. His *'homme-machine'* was a theoretical body, which he tried to construct from the known principles of physics in such a

manner that he could deduce from it the physiological phenomena observed in actual living bodies. In his *Primæ Cogitationes circa Generationem Animalium* he even faced the basic question of machines begetting machines. His physiology was Galenic and Aristotelian, but it was Galen and Aristotle *more geometrica demonstrata*.

Moreover Descartes was not ignorant of the subject at first-hand; he had spent several years studying anatomy, and in *La Dioptrique*, published together with the *Discours* as part of his exemplification of method, himself made a fundamental contribution to the physiology of vision.

'I am resolved to leave all the people here to their disputes,' he said in part 5 of the *Discours*, 'and to speak only of what would happen in a new world, if God were now to create somewhere in the imaginary spaces matter sufficient to compose one, and were to agitate variously and confusedly the different parts of this matter, so that there resulted a chaos as disordered as the poets ever feigned, and after that did nothing more than lend his ordinary concurrence to nature, and allow her to act in accordance with the laws which he had established.' Of the mechanistic theory of the living body, which he claimed to be able to derive from these laws, he said: 'Nor will this appear at all strange to those who are acquainted with the variety of movements performed by the different automata, or moving machines, fabricated by human industry, and with the help of but few pieces compared with the great multitude of bones, muscles, nerves, arteries, veins, and other parts that are found in the body of each animal. Such persons will look upon this body as a machine made by the hands of God, which is incomparably better arranged, and adequate to movements more admirable than in any machine of human invention.'

He gave a detailed account of this theoretical body in his treatise *L'Homme*, which formed part of *Le Monde ou Traité de la Lumière* (completed in 1633 but published posthumously in 1662).

'I assume that the body is nothing more than a statue or machine of clay,' he wrote; 'we see clocks, artificial foun-

tains, mills, and other similar machines which, although made by man, yet have the power of moving themselves in several different ways; and it seems to me that I could not imagine as many kinds of movement in it as I suppose to have been made by the hands of God, nor attribute to it so much artifice that you could not think it could have more . . . I want you to consider next that all the functions which I have attributed to this machine, such as the digestion of food, the beating of the heart and arteries, the nourishment and growth of the members, respiration, waking, and sleeping; the impressions of light, sounds, odours, tastes, heat and other such qualities on the organs of the external senses; the impression of their ideas on the common sense and the imagination; the retention of imprinting of these ideas upon the memory; the interior motions of the appetites and passions; and, finally, the external movements of all members, which follow so suitably as well the actions of objects which present themselves to sense, as the passions and impressions which are formed in the memory, that they imitate in the most perfect manner possible those of a real man; I desire, I say, that you consider that all these functions follow naturally in this machine simply from the arrangement of its parts, no more and no less than do the movements of a clock, or other *automata*, from that of its weights and its wheels; so that it is not at all necessary for their explanation to conceive in it of any other soul, vegetative or sensitive, nor of any other principle of motion and life, than its blood and its spirits, set in a motion by the heat of the fire which burns continually in its heart, and which is of a nature no different from all fires in inanimate bodies.'

In Descartes' theory the body of a human being was occupied by a rational soul. Since the mind was an unextended thinking substance while the body was an unthinking extended substance, some of his critics and followers, such as Gassendi and Malebranche, held that these two substances could have no point of contact. But Descartes held that they interacted through one organ and one only, the pineal gland in the brain (Pl. XV; cf. Pl. XIV; Vol. I, p. 163; below, p. 312 *et seq*.). One reason for his choice

of the pineal gland was that it was the only organ in the brain that was single and not divided into right and left sides. Thus it was adapted to interact with all parts of the body. He held that the cerebral cavity, in which the pineal gland was suspended, contained animal spirits distilled in the heart from the blood, and that through pores in the internal surface of this cavity animal spirits entered the nerves, which he thought were fine hollow tubes. Inside each nerve he held that there were numerous very fine threads, one end of each being attached to a part of the sense organ to which the nerve ran, and the other to a small door at the pore where the nerve reached the internal surface of the brain. The whole nervous function in this machine depended only on the control of the flow of the purely material animal spirits in the brain and nerves, just, he said, as organ music depended only on the control of the air in the pipes.

For example, when light coming from an external object was focussed on to the retina, it pushed a corresponding set of threads in the optic nerve. These in turn opened the corresponding pores on the internal surface of the brain, acting like the wires of a bell-pull. The image formed on the retina was thus reproduced in the pattern of pores opened, and so was traced in the spirits on the surface of the pineal gland. There it was immediately apprehended by the rational soul, which thus received a sensation of the external object. The mind was thus presented with a token of the external world, not the thing in itself.

When, on the other hand, the soul willed a certain action, it acted on the body by moving the pineal gland so that it deflected the animal spirits into the pores opening into the nerves leading to the muscles concerned. The animal spirits acted on the muscle at the end of a nerve by flowing into it and making it swell up, thus causing it to move the limb or part of the body to which it was attached.

By means of this hypothetical model Descartes was able to offer mechanical explanations of many common neurological and psychological phenomena, for example of the co-ordinated control of an action such as walking where many different muscles are involved, of emotions, of images

formed without external objects, of falling to sleep and waking up, of dreams, and of memories, which he held to be the physical traces of the paths of the animal spirits. His explanation of vision and the eye is especially remarkable for its close control by observation and experiment, combined with mathematical analysis of the optical phenomena concerned.

In contrast with man the brutes were simply *automata* and nothing more. Though animals were considerably more complicated, there was no difference in principle between them and the *automata* constructed by human ingenuity. 'There is,' wrote Descartes in a letter to the Marquis of Newcastle, on 23 November, 1646, 'no one of our external actions which can assure those who examine them that our body is anything more than a machine which moves itself, but which also has in it a mind which thinks—excepting words, or other signs made in regard to whatever subjects present themselves, without reference to any passion.' He had said the same thing in the *Discours.* The noises made by animals indicated no such controlling mind and we should not be deceived by their apparently purposive behaviour.

> I know, indeed, that brutes do many things better than we do, but I am not surprised at it; for that, also, goes to prove that they act by force of nature and by springs, like a clock, which tells better what the hour is than our judgment can inform us. And, doubtless, when swallows come in the spring, they act in that like clocks. All that honey-bees do is of the same nature.

The mechanical principles that Harvey had adopted as a method were thus converted by Descartes into a complete philosophy of nature, and just as he had ignored the empiricism of Galileo so he did that of the English physiologist. All three men, however, inspired their successors to bring about the mechanisation of biology. The iatromechanical school adopted the principle that biological phenomena were to be investigated entirely by 'mathematical principles.' The stomach was a retort, the veins and arteries hydraulic tubes, the heart a spring, the viscera

sieves and filters, the lung a bellows and the muscles and
bones a system of cords, struts and pulleys. The adoption
of such conceptions certainly exposed many problems for
investigation by the now established mathematical and ex-
perimental methods, a particularly successful application
being the study of the mechanics of the skeleton and mus-
cular system by Giovanni Alfonso Borelli in his book, *On
the Motion of Animals* (1680). But they were soon carried
to naïve extremes which oversimplified the complexity and
variety of physiological processes, especially of biochemi-
cal processes. Moreover the exhaustiveness of Cartesian
mechanism entirely obliterated biological phenomena that
could not be immediately reduced to them, especially the
apparent purposiveness of animal behaviour (for example,
in the nest-building of birds) and the whole question of
the adaptation of the parts and functions of the body to
each other and of the whole to the environment. These
problems continued to interest naturalists like John Ray
(1627-1704) and they became an important element in
natural theology, proving, not only for Ray but also for
physical scientists like Boyle and Newton, as the title of
Ray's book expressed it, *The Wisdom of God manifested
in the Works of the Creation* (1693). In physiology they
prompted a return to more vitalistic explanations, but it
is a tribute to the power of Descartes' theoretical genius
that the question of vitalism and mechanism continued
until the 20th century to be argued (sometimes uncon-
sciously) in the philosophical terms established by him and
his 17th-century critics.

(4) THE EXTENSION OF MATHE-MATICAL METHODS TO INSTRUMENTS AND MACHINES

As the 17th century progressed, experiment and the use of
mathematics became so intimately linked that such a case
as that of William Gilbert, who had carried on his experi-
mental studies of magnetism almost without mathematics,
would by the end of the century have been almost incon-

ceivable. If causal relations such as those discovered by Gilbert remained incapable of expression in mathematical terms even by Galileo himself, it was generally believed that it was only a matter of time before the problem would be overcome and that this would depend largely on the development of more accurate instruments for measuring.

One of the instruments which Galileo did much to perfect was the clock. At the end of the 15th century the first clocks driven by a spring instead of by weights had been introduced in Nuremberg and this made possible the invention of the portable watch, as, for instance, the 'Nuremberg eggs.' The use of a spring introduced a new problem, for the force it exerted decreased as it became unwound. Various devices were designed to overcome this difficulty, the most successful being the so-called 'fusee' introduced in the middle of the 16th century by the Swiss Jacob Zech. The main principle of this device was to make the driving barrel taper gradually, so that, as the spring became unwound, the loss of force was compensated by an increase in leverage provided by making the spring act on successively wider sections of the barrel. It was still not possible, however, to get a clock that would keep accurate time over a long period. This was becoming a necessity for several purposes, but particularly for the ocean-going navigation that had been expanding since the end of the 15th century. The only practical method of determining longitude depended on the accurate comparison of the time (by the sun) on the ship with that at some fixed point on the earth's surface, for instance Greenwich. Such a clock became possible when a pendulum was introduced as a regulating mechanism. In watches a balance-spring served the same purpose. In 1582 Galileo had discovered that a pendulum swung isochronously, and later saw that this fact might be used in designing a clock. The first accurate clock was invented in 1657 by Huygens, quite independently of Galileo's suggestion. But it was not until the 18th century that the navigational problem was finally overcome, when devices were introduced to compensate for the irregular motion of a ship and for changes in temperature.

Another form of measurement in which the demands of

navigation and travel led to great improvements in the 16th and 17th centuries was the method of making maps. The sensational voyages of Bartholomew Diaz round the Cape of Good Hope in 1486, of Christopher Columbus who reached America in 1492, of Vasco da Gama who reached India in 1497, and of many other sailors who searched for the North-West or the North-East passage, not only added a new world to European consciousness but also made accurate maps and methods of fixing position a fundamental necessity. The essential requirement for mapping the terrestrial globe was a linear measure of the arc of the meridian, for there were few astronomical estimations of latitude, and practically none for longitude, until the 18th century. Various improvements on medieval estimations of the degree were made during 16th and early 17th centuries, but the first accurate figure was given by the French mathematician, Jean Picard, in the second half of the 17th century. In spite of inaccurate figures for the degree, cartography improved greatly from the end of the 15th century. This was in the first place due to a renewed interest in the maps of Ptolemy's *Geography* (see Vol. 1, p. 209). Ptolemy had emphasised the need for the accurate fixing of position, and his maps were drawn on a complete network of parallels and meridians. In the 16th century charts were produced showing much more restricted areas than the medieval charts, and on these rhumb-lines were shown in a simplified form. The compass was used to establish the meridian line, the fact of magnetic variation with longitude being known and taken into consideration. Petrus Apianus, or Bienewitz, whose map, published in 1520, was one of the earliest to show America, in 1524 wrote a treatise on cartographical methods and in another work, *Cosmographicus Liber*, gave a list of latitudes and longitudes of many places in the known world, illustrated with maps (Pl. XVI). Another 16th-century cartographer, Gerard de Cremer or, as he was called, Mercator, of Louvain, in 1569 produced the well-known projection that is still in use showing the spherical earth on a two-dimensional paper. He also experimented with other kinds of projection and he took care to base his maps either on personal surveys, as in his map

of Flanders, or on a critical comparison of the information collected by explorers. The same care was shown by other 16th-century cartographers such as Ortelius, who was geographer to the King of Spain, and Philip Cluvier, who published works on the historical geography of Germany and Italy.

It was in these questions that the governments and administrators of the period showed the greatest interest in science and that the most contact occurred between scientists and mathematicians from the universities, on the one hand, and practical craftsmen—instrument-makers and navigators—on the other. Undoubtedly the most advanced institution concerned with these problems was the long-established *Casa de Contratación*, the great school of navigation at Seville which so much impressed one of the ship's masters of the English explorer Richard Chancellor. But even in a country like England, where in the mid-16th century instrument-makers and pilots were being brought over from the Continent to make up for native backwardness, private enterprise helped to achieve what lack of government patronage left undone. From the second half of the century mathematicians like Robert Recorde, John Dee, Thomas and Leonard Digges, Thomas Hood (employed by Queen Elizabeth's government), Henry Briggs (at Gresham College in London), and Thomas Harriot made efforts to improve mathematical education, especially that of the master-craftsmen, and even gave practical instruction in the new methods of navigation. John Dee, for example, was commissioned to instruct Martin Frobisher's sailing-master before he set out on his first voyage in 1576; Thomas Digges spent several months at sea demonstrating the new methods; and Thomas Harriot accompanied Sir Walter Ralegh's colonists to Virginia in 1585 as 'mathematical practitioner' and adviser.

Essential to accurate cartography on land were accurate surveying methods, and these were improved in the 16th and 17th centuries. The use of the astrolabe, quadrant and cross-staff to measure height and distance was known in the Middle Ages, and in the 16th century Tartaglia and others showed how to fix position and survey land by

compass-bearing and distance. In the late 15th and early
16th centuries very accurate maps were made of Alsace,
Lorraine and the Rhine Valley, notably by Waldscemüller
of Strassburg (1511), in which roads were marked off in
miles and a compass rose was shown. It is thought that
these maps were made with a primitive theodolite known
as the polimetrum. The method of triangulation, by which
a whole country could be surveyed from an accurately
measured base line but otherwise without direct measure-
ment, was first published in print by the Flemish cartog-
rapher, Gemma Frisius, in 1533. In England, the first ac-
curate maps were made by Saxton, at the end of the 16th
century, and Norden, early in the 17th. An outstanding
question which was not settled for some years was the
adoption of a common prime meridian. English cartog-
raphers adopted Greenwich in the 17th century, but it was
not generally accepted until 1925.

The first instrument for measuring temperature seems
to have been invented by Galileo some time between 1592
and 1603, but three other investigators seem independently
to have designed a thermometer, thermoscope, calendar-
glass or weather-glass, as it was variously called, at about
the same time. Galen had represented heat and cold by
a numerical scale and, by the 16th century, though the
senses were the only means of estimating temperature, the
idea of degrees of these qualities had become a common-
place in medical and natural-philosophical literature (see
above, pp. 98–99). The scales of degrees there described,
such as that of eight degrees of each quality, were among
those used for the earliest thermometers. These instru-
ments were themselves adaptations of ancient Greek inven-
tions. Philo of Byzantium and Hero of Alexandria had both
described experiments based on the expansion of air by
heat (see above, p. 37, note), and Latin versions of their
works existed. That of Hero's *Pneumatica* was printed twice
in the 16th century. The first thermometers, which were
adaptations of some of their apparatus, consisted of a glass
bulb with a stem dipping into water in a vessel. Air was
driven out of the bulb by heat and, on cooling, water was
drawn back into the stem. The stem was marked in de-

grees and, as the air in the bulb contracted and expanded, the movement of the water up and down it was held to measure temperature, although, as we now understand, the water would move also with changes in atmospheric pressure.

The attribution of the first invention of this instrument to Galileo rests solely on the testimony of his contemporaries, for it is described in none of his extant works. The first published account of it was given in 1611 in a commentary on Avicenna by the physiologist Santorio Santorii, who used it for clinical purposes. A similar instrument, which seems to have been a modification of Philo's apparatus, was used a few years later by Robert Fludd to demonstrate, according to him, the cosmic effects of light and darkness and heat and cold, to indicate or predict weather conditions, and to measure temperature changes. Another type of thermometer, consisting of a tube with a sealed bulb at each end, seems to have been invented by another contemporary, the Dutchman Cornelius Drebbell (1572-1634). This instrument depended for its operation on the difference in temperature between the air in each bulb, which moved coloured water up or down the stem.

These air thermometers were used for various purposes in the 17th century, though mostly for medical purposes. J. B. van Helmont (1577-1644), for example, used a modification of the open type to take body temperature. They were very inaccurate and the open type was particularly sensitive to changes in atmospheric pressure. The French chemist Jean Rey adapted it in 1632 to form a water thermometer which measured temperature by the expansion and contraction of water instead of air; but technical difficulties prevented the construction of an accurate thermometer until the 18th century.

The desire to measure prompted the invention of an instrument which would give some idea of the weight of the atmosphere, again an instrument for which Galileo was also initially responsible. Such observations as that water would not run out of a water clock while the hole at the top was closed were usually explained, after the 13th century, ei-

ther by Roger Bacon's 'continuity of universal nature' or
in terms of the void (see above, pp. 40–41). Galileo did
not, like the Aristotelians, regard a void as an impossibility.
He produced the earliest recorded artificial vacuum by
drawing a piston from the bottom of an air-tight cylinder
and, like Giles of Rome, he attributed the resistance en-
countered to the 'force of the vacuum.' When he learnt
that a pump would not lift water above about 32 feet, he
assumed that this was the limit of the force. He did not
connect these phenomena with atmospheric weight. In
1643 it was shown, at Torricelli's suggestion, that when a
long tube with one end sealed was filled with mercury and
inverted with its open end under mercury in a vessel, the
length of the column of mercury standing in the tube was
less than that of the water raised by a pump in proportion
to the greater density of mercury. The empty space above
the mercury became known as the 'Torricellian vacuum,'
and Torricelli attributed the effect to the weight of the
atmosphere. Torricelli's apparatus was adapted to form the
familiar J-tube barometer. His conclusions were confirmed
when, under Pascal's direction, a barometer was carried to
the top of the Puy de Dôme and it was found that the
height of the mercury decreased with altitude, that is with
the height of atmosphere above it.

The possibility of creating a vacuum led a number of
scientists during the 16th and 17th centuries to try to de-
vise a practical steam engine. The earliest of these were
driven, in fact, not by the force of expansion of steam but
by atmospheric pressure operating after steam in the cylin-
der had been condensed, though some writers, for instance
de Caus in 1615 and Branca in 1629, suggested using
the turbine device described by Hero of Alexandria, a jet
of steam directed on to a wheel with blades. The most
important practical problem for which steam engines were
suggested was the pumping of water. The problem of keep-
ing the ever-deepening mines free from water became in-
creasingly serious throughout the 16th and 17th centuries.
Agricola in his *De Re Metallica* described several types
of device used for this purpose in the early 16th century:
a chain of dippers worked by a crank turned by hand; a

suction pump worked by a water wheel, with a cam to work the piston and with pipes made of hollow tree trunks clamped with iron bands (Pl. XVII); a force pump worked by a crank; and a rag-and-chain device in which the buckets were replaced by balls of horsehair and the motive power was provided by men walking a treadmill or a horse driving a whim. Pumps were needed also to provide water for fountains, and for town supplies. Augsburg was supplied with water by a series of Archimedean screws turned by a driving shaft which raised the water to the tops of towers, from which it was distributed in pipes; London was supplied after 1582 by a force pump driven by a tide-wheel set up near London Bridge by the German engineer, Peter Morice, and later by other horse-driven pumps; and pumps were used to supply Paris and other towns, and to work the fountains at Versailles and Toledo. As early as the mid-16th century Cardano had discussed methods of producing a vacuum by condensing steam, and in 1560 G. B. della Porta (1536–1605) suggested using a device based on this principle for raising water. This suggestion was put forward again in 1663 by the Marquis of Worcester. The earliest steam engine operating with a cylinder and piston was designed by the French engineer Denis Papin, who had worked with Boyle and invented the condensing pump and also the pressure cooker, or 'steam digester,' as he called it, with a safety-valve. He also designed a steam-driven carriage. A practical steam engine based on the condensation of steam was patented in 1698 by Thomas Savery; it was used in at least one mine and to supply water to several country houses. Hearing of this, Papin in 1707 designed a high-pressure boiler with an enclosed fire-box, and a steam-boat propelled by paddle-wheels. It was his design that Thomas Newcomen a little later successfully adapted for his engine worked by atmospheric pressure; even James Watt's engines were still primarily atmospheric. Towards the end of the 18th century engines were invented which were driven by the expansive force of steam at high pressure.

The Torricellian vacuum was taken as a final refutation of Aristotle's arguments against the existence of void

which, according to some of his followers, 'nature ab-
horred.' The arguments against the void, drawn from the
Aristotelian law of motion, had already been disposed of
by Galileo. But Aristotle himself had sometimes confused
arguments against the existence of void, in the sense of
'non-being,' with physical arguments against, for instance,
the absence of a resisting medium. Many of his 17th-
century critics did the same. The Torricellian vacuum was
not an ontological void such as Descartes, among others,
could not accept. It was a space which, at least theoreti-
cally, contained no air or similar matter. Indeed, although
later physicists were not so sensitive to metaphysical ni-
ceties as Descartes, they found it necessary to postulate a
plenum of some sort, and this continued to play a variety
of physical roles down to the 20th century. Torricelli
showed that light was transmitted though a vacuum and,
beginning with Gilbert's effluvia, 17th-century physicists
filled up the void with a medium, the ether, capable of
propagating all the known influences such as gravity, mag-
netism and light. Descartes himself attempted to explain
magnetism by vortices which, like Averroës' *species mag-
netica,* entered by one pole of the magnet and left by the
other. He held that these acted on iron because the re-
sistance of its particles to the flow drew it to the magnet
(Pl. XVIII). Non-magnetisable substances did not offer
such resistance.

Instruments designed for closer observation as well as
for more accurate measurement were also constructed dur-
ing the 17th century, the most important being the tele-
scope and the compound microscope. The propagation of
light was still explained by most 16th-century opticians in
terms of the 'species' theory, which was in keeping with
the geometrical discussions of the time. In the 16th century
the first published attempt at a geometrical analysis of
lenses and the eye was that by Maurolyco. He denied that
the lens was the seat of vision, but could not understand
the inverted image. It was in fact Averroës who first rec-
ognised the true functions of the lens and retina, but these
seem to have been forgotten until they were reasserted by
the anatomist Felix Plater (1536–1614). The anatomists

Realdo Colombo and Hieronymo Fabrizio first drew the lens in the front part of the eye and not, as had been done previously, in the middle. Kepler in his commentary on Witelo (1604) first demonstrated that the rays focussed by the cornea and lens formed a real inverted image on the retina.

A convenient method of isolating stars by observing them through a tube had already been introduced by the Arabs and, with the spread of spectacles, the lens-grinding industry had developed in a number of centres. Pioneer work on combinations of mirrors and perhaps lenses was carried out, apparently under the inspiration of Roger Bacon, by the English mathematicians Leonard Digges (d. c. 1571) and his son Thomas, but they set up their apparatus on frames, without tubes. It seems that some sort of telescope with lenses in a tube was constructed in Italy about 1590. In any case it is recorded that a Dutch spectacle-maker named Janssen in 1604 copied an Italian model marked with that date, and the record gives point to Porta's obscure account in 1589 of a combination of convex and concave lenses. For some reason Galileo only heard of the Dutch instruments, and he then constructed his telescope and compound microscope from his scientific knowledge of refraction.[19] He did not fully understand this phenomenon and Kepler, in his *Dioptrica* (1611), gave a more intelligible theory. Galileo's combination of concave and convex lenses was replaced by combinations of convex lenses, and in the course of time rules were worked out for determining focal lengths and apertures. The true law of refraction, that the ratio of the sines of the angles of incidence and refraction is a constant de-

[19] When the Frenchman, Jean Tarde, called on Galileo in 1614, he said, 'Galileo told me that the tube of a telescope for observing the stars is no more than 2 feet in length; but to see objects well, which are very near, and which on account of their small size are hardly visible to the naked eye, the tube must be two or three times longer. He tells me that with this long tube he has seen flies which look as big as a lamb, are covered all over with hair, and have very pointed nails, by means of which they keep themselves up and walk on glass, although hanging feet upwards.' Galileo, *Opere*, Ed. Naz., Vol. 19, p. 589.

pending on the media concerned, was discovered only a few years before 1626 by another Dutchman, Willibrord Snell (1591–1626). The law was formulated, probably in the first instance independently, by Descartes, who gave it its first publication in his *Dioptrique* in 1637.

Descartes attempted to conceive of the physical nature of light in a more strictly mathematical form than his predecessors. In accordance with his own mechanical principles he held that light consisted of particles of the *plenum* and that it was transmitted instantaneously by mechanical pressure from one particle to the next. Colour he held to depend on the different rotary velocities of the particles. When giving 'Snell's law' he presented it as a deduction from this conception of the mechanical nature of light, and in his *Météores* (1637) he tried to use this law to explain the two phenomena exhibited by the rainbow, the bright circular bow and the colours. Theodoric of Freiberg's diagrams of the formation of the primary and secondary bows, showing the essential fact of the internal reflection of the sunlight in the raindrops, had been published in Erfurt in 1514, and Antonio de Dominis had given a somewhat inaccurate report of a similar explanation in 1611 (see Vol. I, pp. 110–11). This was almost certainly known to Descartes, if he did not know Theodoric's own diagrams. But Descartes' knowledge of the law of refraction and of optics in general made his treatment of the subject altogether superior to that of his predecessors. He not only gave a complete account of the refraction and reflection of the rays in the water drops causing the rainbow, but also showed that those coming to the eye at an angle of about 41 degrees from their original direction from the sun were much more dense than those coming from other directions and so produced the primary bow. He definitely associated the colours with differential refrangibility, which he explained by his theory of rotating particles. Some time later Johann Marcus Marci of Kronland (1595–1667) showed that rays of a given colour were dispersed no further by a second prism. Neither Descartes nor Marcus was able to produce an adequate theory of colour, which had to wait until their experiments with prisms had been re-

peated and extended by Newton, with again an altogether superior theoretical understanding of the question. This 17th-century work of Descartes, Newton, Hooke, Huygens and others on light made it possible for serviceable microscopes and telescopes to be constructed, but the usefulness of both these instruments was somewhat reduced by the failure to overcome the chromatic aberration, which became serious with powerful lenses. With telescopes the problem of getting a large magnification was overcome by using concave mirrors instead of lenses, but a really powerful microscope became possible only in the 19th century.

(5)   CHEMISTRY

In chemistry, such progress as was achieved by the middle of the 17th century was the result rather of experiment and observation alone than of interpretation of facts in terms of mathematical generalisations. The expansion of alchemy and the pursuit of more strictly practical ends, such as painting and mining, had led, during the 14th and 15th centuries, to a fairly wide familiarity with ordinary chemical apparatus. Although this had included the balance, this instrument had not, as Cusa had suggested, been combined with *inventio*, or discovery, and the 'art of latitudes' for the development of a quantitative chemical theory. Mineral drugs had begun to come into pharmaceutical and medical practice, and through an extended study of them chemistry was given a marked impetus during the early decades of the 16th century by the bizarre Philippus Aureolus Theophrastus Bombastus von Hohenheim, or Paracelsus (1493–1541). Paracelsus was an accomplished experimenter and added a few facts to chemical knowledge, for instance the observation that while the vitriols were derived from a metal and the alums were derived from an 'earth' (metallic oxide). He also contributed the *tria prima*, sulphur, mercury and salt, to chemical theory. The Arabs had held that sulphur and mercury were the chief constituents of metals, but Paracelsus made sulphur (fire, the inflammable principle), mercury (air, the fusible

and volatile principle) and salt (earth, the incombustible and non-volatile principle) the immediate constituents of all material substances. The ultimate constituents of matter, of which these *tria prima* were themselves composed, were the four Aristotelian elements. He illustrated his theory by burning wood, which gave off flames and fumes and left ash.

The chief influence that Paracelsus had on chemistry was through his assertion that its main business was not with the transmutation of metals, though he held this to be possible, but with the preparation and purification of chemical substances for use as drugs. After him, chemistry became an essential part of medical training, and for nearly a century doctors were divided into paracelsists (or 'spagyrists') and herbalists, who kept to the old herbal drugs. The former were often very incautious in their remedies but, however disastrous for the patient, the contribution of iatrochemistry (medical chemistry) to chemistry itself is well illustrated by the clear and systematic account of techniques and substances given in the *Alchymia* (1597) of Andreas Libavius (1540–1616). Like the practical manuals of Vanoccio Biringuccio (1480–1539), Agricola and Bernard Palissy (1510–c. 1590) in other aspects of the subject, Libavius' book shows the progress of the 16th century in the collection of fact.

The first serious improvements in method, aimed at the chemical analysis of the nature of matter, were made by Johann Baptista van Helmont. After graduating in medicine at Louvain, van Helmont made a wealthy marriage and settled down to the charitable practice of his profession and research in his laboratory. His writings, which he left unpublished, were collected after his death and published by his son under the title *Ortus Medicinæ*. An English translation, *Oriatrike or Physick Refined,* appeared in 1662. Van Helmont's empiricism showed the influence both of the practical chemists who had preceded him and, in spite of his attacks on the schools, of nominalism and Augustinian-Platonism. He held that the sources of human knowledge were both Divine illumination and sensory experience. "The meanes of obtaining Sciences, are

onely to pray, seek and knock,' he said in the tract *Logica Inutilis* which forms chapter 6 of the *Oriatrike*. In the study of nature there was no true *inventio*, or discovery, but 'by bare observation' of concrete and measurable objects.

> For when anyone sheweth me *lapis Calaminaris*, the preparing of *Cadmia* or *Brasse Oare*, the content of, or what is contained in Copper, the mixture and uses of *Aurichalcum*, or *Copper* and *Gold*, which things I knew not before, he teacheth, demonstrateth, and gives the knowledge of that, which before there was ignorance of.

But the logic of the school philosophers could not lead to such discoveries. By itself 'Logical invention is a meer re-taking of that which was known before.' After observation had been made, the investigator was led by *ratio*, that is formal logic and mathematics, to a knowledge of the active principles, which in effect were analogous to the Aristote-lian substantial form, and were the source of the observed behaviour. But, van Helmont said, unless such reasoning was accompanied by intuition or illumination its conclusions were always uncertain.

Van Helmont made this theory of knowledge the basis of a suggested reform of education. 'Certainly I could wish,' he said in the *Oriatrike*, chapter 7, referring to the schools' teaching of Aristotle and Galen,

> that in so short a space of life, the Spring of young men, might not be hereafter seasoned with such trifles, and no longer with lying Sophistry. Indeed they should learn in that unprofitable three years space, and in the whole seven years, Arithmetick, the Science Mathematical, the Elements of Euclide, and then Geographie, with the circumstances of Seas, Rivers, Springs, Mountains, Provinces, and Minerals. And likewise, the properties, and Customs of Nations, Waters, Plants, living Creatures, Minerals, and places. Moreover, the use of the Ring, and of the Astrolabe. And then, let them come to the Study of Nature, let them learn to know and separate the first Beginnings of Bodies . . . And all those things,

not indeed by a naked description of discourse, but by handicraft demonstration of the fire. For truly, nature measureth her works by distilling, moystening, drying, calcining, resolving, plainly by the same meanes, whereby glasses do accomplish those same operations. And so the Artificer by changing the operations of nature, obtains the properties and knowledge of the same.

Van Helmont held that there were two 'first beginnings' of bodies. He had performed Cusa's experiment with the willow (see above, p. 99), and this convinced him that the ultimate inert constituent of material substances was water. The active principle which disposed the water and constructed the specific concrete thing was a 'ferment or seminal beginning,' which was generated in matter by the Divine light (or celestial influence). This last brought the 'archeus,' the efficient cause enabling the ferment to construct the 'seed' which developed into a stone, metal, plant or animal. 'For,' as he said in the *Oriatrike*, chapter 4,

> the seminal efficient cause containeth the Types or Patterns or things to be done by itself, the figure, motions, houre, respects, inclinations, fitnesses, equalizings, proportions, alienation, defect, and whatsoever falls in under the succession of dayes, as well in the business of generation, as of government.

Such bodies were constructed in accordance with the 'idea' of the archeus. In the generation of animals the *archeus faber* of the male seed epigenetically constructed the embryo out of the materials provided by the female. Seeds of organic origin were not, however, indispensable for generation, and perfect animals might be produced when the archeus acted on a suitable ferment. Indeed, van Helmont held that the parent was only equivocally the efficient cause of the offspring. It was the 'natural occasion' on which the seed was produced, but the effective efficient cause was God. This theory was similar to that of the 'occasionalists' (see below, pp. 313–14). He held that there were only two causes operating in natural events, the material and the efficient.

Van Helmont held that there were specific ferments and archei in the stomach, liver and other parts of the body; these controlled their functions, on which his views were in general Galenic. He held also that a disease was an alien entity imposing its way of life, or archeus, on that of the patient; and in developing this idea he became a pioneer in ætiology and morbid anatomy. By putting into practice the doctrine that knowledge of the ferments was to be derived from observation of their material effects, he was able also to assign specific functions to many of the Galenic and other principles. He demonstrated the acid digestion, or 'fermentation,' in the stomach, and its neutralisation by the bile. These, he said, were the first two fermentations of the food passing through the body. The third took place in the mesentry; the fourth was in the heart, where the red blood became more yellow by the addition of vital spirits; the fifth was the conversion of arterial blood into vital spirit, mainly in the brain; the sixth was the elaboration of the nutritive principle in each part of the body from the blood. Van Helmont also anticipated something like the principle of the specific energy of nerves when he said that vital spirit conveyed to the tongue accounted for the perception of taste, but would not cause taste in the finger.

In pure chemistry, van Helmont made systematic use of the balance and demonstrated the conservation of matter, which, he held, secondary causes could not destroy. He showed that if a certain weight of silica were converted into waterglass and the latter were then treated with acid, the precipitated silicic acid would on ignition yield the same weight of silica as that originally taken. He showed also that metals dissolved in the three main mineral acids could be recovered again, and realised that when one metal precipitated another from a solution of a salt this did not, as Paracelsus had thought, imply transmutation. Perhaps his most important work was on gases. He himself coined the name 'gas' from the Greek *chaos*. Several medieval and later writers had recognised the existence of aqueous and earthy 'exhalations' as well as air, but van Helmont was the first to make a scientific study of different kinds of

gases. Here his research was made much more difficult by the lack of a convenient apparatus for collecting gases. The different kinds of gas he mentioned included a *gas carbonum* given off by burning charcoal (usually carbon dioxide but also carbon monoxide); a *gas sylvester* given off by fermenting wine, by spa water, and by treating a carbonate with acetic acid, and also found in certain caves, which put out a flame (carbon dioxide); a red poisonous gas, which he also called *gas sylvester*, given off when aqua fortis acted on such metals as silver (nitric oxide); and an inflammable *gas pingue* formed by dry distillation of organic matter (a mixture of hydrogen, methane and carbon monoxide). Van Helmont also took an interest in respiration, of which he maintained the purpose to be not, as Galen had said, to cool but to maintain animal heat; this it did by a ferment in the left ventricle which changed the arterial blood into vital spirit.

Several other chemists made experiments with gases during the early decades of the 17th century in connection with the phenomena of combustion. According to the accepted theory, combustion involved the decomposition of compound substances with the loss of the inflammable 'oily' principle present in the 'sulphur.' Burning would thus result in a decrease in weight. Several observations were made, however, which led to the development of new ideas on this subject. The experiment of 'enclosed combustion,' in which a candle was lighted in a glass upturned in a basin of water, had been described by Philo (see above, p. 37, note), and Francis Bacon referred to it as a common experiment. It was repeated by Robert Fludd (1617), and when the water rose as the air was consumed he described the latter as 'nourishing' the flame. It had also been known by both Arab and 16th-century chemists that during calcination metals increased in weight. In 1630 Jean Rey gave reasons for believing that the definite and limited 'augmentation' in weight, which he observed in the calx of lead and tin, could have come only from the air, which he said mixed with the calx and became attached to its most minute particles. He maintained, further, that all elements, including fire, had weight and that this weight was

conserved throughout chemical changes. These facts and ideas were clearly incompatible with the theory of the 'oily' principle, and when this principle was developed as 'phlogiston' it had to be considered as having negative weight. But it was not until towards the end of the 18th century that combustion was firmly associated with oxidation, when it became the central question of the Chemical Revolution initiated by Lavoisier and his contemporaries.

The universal mechanism which accompanied the successes of mathematical physics entered chemistry through the development of the atomic theory. Such natural philosophers as Bruno, who had argued for the actual existence of natural or physical *minima*, continued the scholastic discussions of this problem, and it was given prominence by Francis Bacon who, though he changed his mind later, began with a favourable opinion of atoms and also said that heat was a condition produced by the vibration of corpuscles. Galileo said that change of substance 'may happen by a simple transposition of parts.' The first application of the atomic theory to chemistry was made by the Dutchman Daniel Sennert (1572–1637). Sennert maintained that substances subject to generation and corruption must be composed of simple bodies, from which they arose and into which they were resolved. These simple bodies were physical and not merely mathematical *minima*, and were in fact atoms. He postulated four different kinds of atoms, corresponding to each of the four Aristotelian elements, and elements of the second order (*prima mixta*) to which the Aristotelian elements gave rise when combined. He held that atoms, for example of gold in solution in acid and of mercury in sublimation, retained their individuality in combination, so that the original substances could be regained from compounds. Similar ideas were expressed by Joachim Jung (1587–1657), through whom they later became known to Robert Boyle (1627–91).

Contributions to the atomic theory were made also by Descartes, for although he did not believe in indivisible physical *minima*, he tried to extend his mechanistic principles to chemistry by attributing the properties of various substances to the geometrical shapes of their constituent

earthy particles. For instance, he supposed the particles of corrosive substances such as acids to be like sharp-pointed blades, while those of oils were branching and flexible. These ideas were used later by John Mayow (1643–79), and they became familiar to chemists through the *Cours de Chymie* (1675) by Nicolas Lémery (1645–1715). Another geometer, Gassendi, popularised the atoms of Epicurus (1649), maintaining, however, that they had not been in existence since eternity but had been created with their characteristic powers by God. He based his belief in the existence of void on Torricelli's experiments and, like Descartes, connected chemical properties with the shapes of the atoms. He also attributed combination into *moleculæ* or *corpuscula* to mechanisms such as hooks and eyes. Gassendi's system was the subject of an English work by Walter Charleton (1654), physician to Charles II and an early fellow of the Royal Society. The microscope had lent an interest to discovering the size of atoms and Charleton argued, from such phenomena as volatilisation and solution, that the smallest discernible microscopic particle contained ten hundred thousand million invisible particles. Through Charleton the atomic theory became well known in mid-17th-century England. When it was adopted by Boyle and Newton, the empirical conceptions of van Helmont and the earlier practical chemists were transformed in accordance with mechanical principles, and chemistry, like physics, finally set out on its course of being reduced to a mathematical science. After the discovery of 'combining weights' and Dalton's generalisation of the results in his atomic theory early in the 19th century, the fulfilment of that process became inevitable.

(6) BOTANY

Botanical studies up to the middle of the 17th century were confined principally to the business of collecting and classifying facts, and were left almost untouched by the mathematical revolution in scientific thought. In fact, even in the 20th century, botany, like many other branches of

biology, remains singularly intractable to mathematical treatment. The theory in which the animate world eventually found a universal explanation, the theory of organic evolution, was based on logical rather than mathematical abstractions.

The dual interest of medical men in descriptive botany and anatomy, which continued into the 16th century, brought it about that these were the first aspects of biology to develop and that this was almost entirely the work of medical men. It was customary in some places, such as Montpellier, to take up botany in summer and anatomy in winter. The first books on scientific botany to be printed were nearly all herbals. The best of these, such as the *Latin Herbarius* (1484), which had probably already existed in manuscript, and the *German Herbarius* (1485), besides being compilations from classical, Arabic and medieval Latin authors, also included descriptions and illustrations of local, for instance German, plants. Rufinus, the best of the known medieval Latin herbalists, seems, however, to have been forgotten.

Besides the medical interest in identifying plants for use as drugs, 16th-century doctors shared with lexicographers the humanist interest in identifying the plants mentioned in the recently printed Latin editions of Pliny (1469), Aristotle (1476), Dioscorides (1478), and Theophrastus (1483). More than one humanist naturalist, of whom the Swiss Conrad Gesner (1516–65) is a typical example, began by trying to find and identify in his own country, for purposes of textual criticism, the plants and animals mentioned by classical authors; and out of this developed an interest in local fauna and flora for their own sake. The extraordinary interest which animals, plants and rocks were arousing among such people by the middle of the 16th century is shown by the enormous correspondence on the subject, with descriptions of local expeditions and the transmission of specimens, drawings and descriptions, carried on by Gesner and other naturalists. It was soon realised, as indeed Albertus Magnus and Rufinus had known well, that there were other creatures in existence besides those known to the ancients. The classical limitations were

finally destroyed by the new fauna, flora, foods and drugs coming to Europe from the New World and the East. Plants and animals were then described and drawn for their own sakes and called by their common vernacular names to a large extent without reference to the classics.

The first result of this 16th-century botanical activity, which was greatest in Germany, the Netherlands, southern France and Italy, was to increase the number of individual plants known. Lists of local flora and fauna were drawn up for various districts. Botanical gardens, which had long been kept by monasteries and, from the 14th century, had been planted by some medical schools, were established in various further university towns such as Padua (1545), Bologna (1567) and Leyden (1577). The last two were presided over respectively by Aldrovandi and Cesalpino, and by de l'Ecluse. Others were established later at Paris (1620), Oxford (1622) and other places. The practice of preserving dried plants, 'dry gardens,' which began in Italy, also allowed botany to go on in the winter months. At the same time the Portuguese herbalist Garcia da Orta published a book on Indian plants at Goa (1663), and the Spaniard Nicolas Monardes published the first descriptions of 'el tabaco' and other American plants (1569–71).

In the northern school, whose interest was purely floristic, a continuous development of botanical ideas may be traced from the four 'fathers' of German botany to Gaspard Bauhin. For all the members of this school the primary intention was simply to make it possible to identify individual wild and cultivated plants and distinguish them from those resembling them. This led to concentration on accurate illustrations and descriptions. The illustrations, which in the herbal of Otto Brunfels (1530), the first of the German fathers, were made by Hans Weiditz, an artist of the school of Albrecht Dürer (1471–1528), were at first greatly superior to the pedantic traditional descriptions. With Jerome Bock (1539) and Valerius Cordus (1561) the latter began gradually to improve. The object of both illustrations and descriptions was simply to depict the most easily recognisable aspects of external appearance, such as the form and disposition of roots and branches, the shape

of the leaves, and the colour and shape of the flowers (Pl. XIX). There was no interest in the comparative morphology of the parts. For instance, the glossary of terms given by the third German father, Leonard Fuchs (1542), referred almost entirely to such general characters; and the earlier attempts at classification, for instance those made by Bock and the Netherlander Rembert Dodœns (1552), were based for the most part on artificial characteristics such as edibility, odour or medicinal properties.

Since the task of describing individual forms necessarily involved distinguishing them from near relations, some appreciation of 'natural' affinity was inevitable. Gesner, whose botanical work unfortunately was not published until long after his death and thus apparently had little or no influence on his contemporaries, distinguished different species of a given genus, for example Gentian, and also seems to have been the first to draw attention to the flower and fruit as diagnostic characters. Other writers, such as Dodœns and Charles de l'Ecluse (1576), though primarily interested simply in giving order to their work, placed together within each artificial division plants belonging to what are now recognised as natural groups. This practice had·been carried even further by Mathias de Lobel (1571), like de l'Ecluse a graduate of Montpellier, who had based his classification mainly on leaf structure. It reached its final stage in Gaspard Bauhin (1560–1624), professor of anatomy at Basel. Bauhin's descriptions are precise and diagnostic, as may be seen from that of the beet, which he called *Beta Cretica semine aculeato*, given in his *Prodomus Theatri Botanici* (1620).

> From a short tapering root, by no means fibrous, spring several stalks about 18 inches long: they straggle over the ground, and are cylindrical in shape and furrowed, becoming gradually white near the root with a slight coating of down, and spreading out into little sprays. The plant has but few leaves, similar to those of *Beta nigra*, except that they are smaller, and supplied with long petioles The flowers are small, and of a greenish yellow. The fruits one can see growing in large num-

bers close by the root, and from that point they spread
along the stalk, at almost every leaf. They are rough and
tubercled and separate into three reflexed points. In
their cavity, one grain of the shape of an *Adonis* seed is
contained; it is slightly rounded and ends in a point, and
is covered with a double layer of reddish membrane, the
inner one enclosing a white, farinaceous core.

The number of plants described by Bauhin had in-
creased to about 6,000, as compared with the 500 or so
given by Fuchs. He systematically used a binomial nomen-
clature, though he did not invent this system for it had
occurred in a 15th-century manuscript of the *Circa Instans*.
In his *Pinax Theatri Botanici* (1623), he gave an exhaus-
tive account of the synonyms used by earlier botanists. In
enumerating the plants described, he proceeded, as de
Lobel had done, from supposedly less perfect forms, such
as grasses and most of the Liliaceæ, through dicotyledon-
ous herbs to shrubs and trees. Both he and de Lobel thus
made a practical distinction between monocotyledons and
dicotyledons and, as some of their predecessors had done
in varying degrees, put together plants belonging to such
families as the Cruciferæ, Umbelliferæ, Papilionaceæ,
Labiateæ, Compositæ, etc. Such grouping was, however,
based entirely on an instinctive appreciation of likeness
in form and habit. There was no conscious recognition
of comparative morphology, and no system was set out
based on the understanding and analysis of morphological
features. The main effort of the northern school was in fact
towards the accumulation of more and more empirical
descriptions, until by the end of the 17th century John
Ray (1682) was able to cite 18,000 species.

The man who made it possible to reduce this mass of
information to some sort of rational order was the Italian
Andrea Cesalpino, professor of medicine first in Pisa and
then in Rome, where he was also physician to Pope Clem-
ent VIII. Cesalpino brought to the study of botany not
only the floristic knowledge of the herbalists, but also an
interest in the detailed morphology of the separate parts of
the plant and an Aristotelian mind capable of forming gen-

eralisations. He based his attempt, set out in the *De Plantis* (1583), to explain the 'real' or 'substantial' affinities between plants on the Aristotelian principle that the final cause of vegetative activity was nutrition, of which the reproduction of the species was simply an extension. In his day the role played by the leaves in nutrition was still unknown, and the nutritive materials were supposed to be absorbed by the roots from the soil and carried by the veins up the stem to produce the fruit. The centre of vital heat, corresponding to the heart in animals, was the pith, and Cesalpino held that it was also from the pith that the seeds were produced. The co-operation of the male and female parts of flowers in reproduction had not yet been discovered, and he supposed that the flower was simply a system of protecting envelopes round the seed, comparable to the foetal membranes of animals. On these principles he divided plants first, according to the nature of the stem conducting the nutritive materials, into woody and herbaceous plants and again, within these groups, according to the organs of fructification. Here he began with plants such as fungi, which he held had no seed but were spontaneously generated from decaying substances, and passed through others such as ferns, which propagated by a kind of 'wool,' to plants with true seeds. He then classified these last according to the number, position and shape of the parts of the fruit, with sub-divisions based on root, stem and leaf. Characteristics such as colour, odour, taste or medicinal properties he considered to be mere accidents.

Cesalpino's attempt to deduce a 'natural' classification from the principles he had assumed was in result deplorable. The distinction between monocotyledons and dicotyledons was less clear than with the herbalists and, out of the 15 classes he made, only one, the Umbelliferæ, corresponds to what would now be recognised as a natural group. Nevertheless, his system was based on considerable knowledge and clear principles which, however wrong, were the first to be introduced by botanists of the time into the study of plants. His followers had something to work on. The first to criticise and develop Cesalpino's ideas was Joachim Jung (1587–1657), a German professor of medi-

cine who probably came across his ideas while studying at Padua. Jung accepted the idea that nutrition was the fundamental vegetative function and, like Cesalpino, based his idea of species on reproduction. He made what was then a great advance by discussing morphology as far as possible in independence of physiological questions.

Theophrastus, whose *Historia Plantarum* had been translated into Latin by Theodore of Gaza (1483), had given morphological descriptions of the external parts of plants from root to fruit. He had also set forth the 'homology' of the perianth members of flowers, watched the development of seeds, and to some extent distinguished between monocotyledons and dicotyledons. His interests had been by no means confined to morphology. He had made an attempt to understand the relation between structure and function, habits and geographical distribution, had described the fertilisation of the date palm, and had tried to understand the caprification of the fig, though the flowers were distinguished only by Valerius Cordus. Theophrastus had also established the first rudiments of plant nomenclature, and there was practically no further development in the subject until similar morphological descriptions and distinctions were made by Jung.

Jung's precise definitions of the parts of plants, for which he made use of the logical refinements developed by the later scholastics and of his own mathematical gifts, were the foundation of subsequent comparative morphology. For instance, he defined the stem as that upper part of the plant above the root which stretched upwards in such a way that back, front and sides could not be distinguished, while in a leaf the bounding surfaces of the third dimension (apart from the length and breadth) in which it was extended from its point of origin were different from one another. The outer and inner surfaces of a leaf were thus differently organised and this, as well as the fact that they fell off in autumn, enabled compound leaves to be distinguished from branches. Botanists were not yet ready to follow this lead, and neither Jung nor Cesalpino had much effect on their contemporaries, who continued to devote their energies to empirical descriptions. It was only at the

end of the 17th century that botanists once more recognised the need for a 'natural' system of classification and attempted to base it on comparative morphology. The culmination of their effort was the system of Linnæus (1707–78), who acknowledged his debt to both Cesalpino and Jung. When the 'natural' classification came itself to call for an explanation this was supplied by the theory of organic evolution.

(7) ANATOMY AND COMPARATIVE ANIMAL MORPHOLOGY AND EMBRYOLOGY

The great advances made in anatomy and zoology during the 16th and early 17th centuries were, like those in botany, due simply to a new precision of observation and remained largely untouched by mathematics. Just as 16th-century botany began with the object of identifying medicinally useful plants, so anatomy began with such aspects as would facilitate the work of surgeons and artists. What the practical needs of the surgeon chiefly required were good topographical descriptions; comparative morphology had little interest for him. The painters and sculptors, of whom several, such as Andrea Verrocchio (1435–88), Andrea Mantegna (d. 1516), Leonardo da Vinci, Dürer, Michelangelo (1475–1564) and Raphæl (1483–1520), are known to have used the scalpel, required little more than surface anatomy and a knowledge of bones and muscles. As the century went on, however, a greater practical interest was taken both in functional questions and in the structure and habits of animals. In both developments by no means the least important factor was the brilliant revolution brought about by the artists themselves in anatomical illustration.

The artist who has left most evidence of his anatomical exercises is Leonardo da Vinci and, as in mechanics, his researches went far beyond the practical needs of his craft. He even planned a text-book of anatomy in collaboration with the Pavian professor Marcantonio della Torre (c. 1483–1512), who died before the book was written.

A.G. 2.—K

Leonardo was guided by earlier text-books and repeated some of the old mistakes, such as drawing the lens in the centre of the eye (cf. Pl. XX). His claim always to have followed experience may be accepted in the same spirit as the same claim made by many of his predecessors. He made several original observations on both human and comparative anatomy, and carried out physiological experiments which were often fruitful and always ingenious. He was one of the first to make use of serial sections. The animals which he mentioned as the subjects of his researches include *Gordius*, moths, flies, fish, frog, crocodile, birds, horse, ox, sheep, bear, lion, dog, cat, bat, and monkey. His best figures were of bones and muscles, those of the hand and shoulder being clear and substantially accurate. Others exhibited the action of muscles. He made models with bones and copper wire, and pointed out that the power of the biceps brachii depended on the position of its insertion with respect to the hand. He compared the limbs of man and horse, showing that the latter moved on the tips of its phalanges. He studied the wing and foot of the bird, the mechanics of flight, and the operation of the diaphragm in breathing and defæcation. He studied the heart and blood vessels. He also made good drawings of the placenta of the cow, but was uncertain whether the maternal and fœtal blood streams were connected or not. One of his most ingenious feats was to make wax casts of the ventricles of the brain. He also carried out experiments on the spinal cord of the frog, and concluded that this organ was the 'centre of life.'

Leonardo made a further contribution to biology, as well as to geology, when he used inland shells to support Albert of Saxony's theory of the formation of mountains (Vol. I, p. 128). 'Why,' he asked, 'do we find the bones of great fishes and oysters and corals and various other shells and sea-snails on the high summits of mountains by the sea, just as we find them in low seas?'[20]

There had been a continuous interest in local geology in Italy since the 13th century, and in his speculations on

geology Leonardo made use of his own observations on the
sea coast, the Alps and its streams, and Tuscan rivers such
as the Arno. He rejected the theories that fossils were not
the remains of living things but were accidents or 'sports'
of nature or had been spontaneously produced by astral
influence, and that they were organic remains which had
been transported from elsewhere by the Flood. He accepted
instead Avicenna's theory of fossil formation which he had
learnt from Albertus Magnus. He then maintained that the
arrangement of shells in strata, with gregarious forms such
as oysters and mussels in groups and solitary forms apart
just as they were found living on the seashore, and with
crabs' claws, shells with those of other species fastened to
them, and bones and teeth of fish all mixed up together,
suggested that fossils were the remains of animals which
had formerly lived in the same place just as contemporary
marine animals did. The mountains on which the shells
were found had formerly formed the sea floor, which had
been, and was still being, gradually raised by the deposit
of river mud.

> The shells, oysters and other similar animals which
> originate in sea-mud, bear witness to the changes of the
> earth round the centre of our elements. This is proved
> thus: Great rivers always run turbid, owing to the earth,
> which is stirred by the friction of their waters at the
> bottom and on their shores; and this wearing disturbs
> the face of the strata made by the layers of shells, which
> lie on the surface of the marine mud, and which were
> produced there when the salt waters covered them; and
> these strata were covered again from time to time with
> mud of various thicknesses, or carried down to the sea by
> the rivers and floods of more or less extent; and thus
> these shells remained walled in and dead underneath
> these layers of mud raised to such a height that they
> came up from the bottom to the air. At the present
> time these bottoms are so high that they form hills or
> high mountains, and the rivers, which wear away the
> sides of these mountains, uncover the strata of these
> shells, and thus the softened side of the earth continually

rises and the antipodes sink closer to the centre of the
earth, and the ancient bottoms of the sea have become
mountain ridges.[21]

The surgical developments of the 15th century, which
received fresh impetus from the printing of the *De Medi-
cina* of Celsus in 1478, first issued an anatomical discovery
with Alexander Achillini's (1463–1512) description, in his
commentary on Mondino, of 'Wharton's duct,' of the entry
of the bile duct into the duodenum, and of the hammer
and anvil bones of the middle ear. The clear influence of
naturalistic art on anatomical illustration is first seen in
the Italian work, *Fasciculo di Medicina* (1493), while
Berengario da Carpi (d. 1550), professor of surgery in
Bologna, was the first to print figures illustrating his text.
In his commentary on Mondino (1521), Berengario also
described a number of original observations. He demon-
strated experimentally that the kidney is not a sieve, for
when injected with hot water from a syringe it merely
swelled up and no water passed through. He showed in a
similar way that the bladder of a nine months' unborn
child had no opening other than the urinary pores. He also
denied the existence of the *rete mirabile* in man, gave the
first clear accounts of the vermiform appendix, the thymus
gland and other structures, had some idea of the action of
the cardiac valves, and coined the term *vas deferens*. An-
other surgeon of the same period who had a good practical
knowledge of anatomy was Nicholas Massa, who published
a work on the subject in 1536. The first to publish illustra-
tions showing whole venous, arterial, nervous or other sys-
tems (1545) was Charles Estienne (1503–64), of the well-
known family of French humanist printers. He also traced
the blood vessels into the substance of the bone, noted the
valves in the veins, and studied the vascular system by in-
jecting the vessels with air. Another work which illustrates
the advances in anatomy made during the early decades of
the 16th century is the tract published by Giambattista
Canano (1515–79) in 1541, in which he showed each mus-
cle separately in its relations with the bones.

[21] Richter, vol. 2, pp. 146–47.

Besides these improvements in knowledge of anatomy, a number of purely empirical advances were made in practical surgery in the 16th century. One of the greatest problems for an army surgeon was how to treat gunshot wounds. At first these were believed to be poisonous and were treated by scalding with oil of elders, with terrible results. One of the first doctors to abandon this practice was Ambroise Paré (1510–90), who described in his fascinating *Voyages en Divers Lieux* how he had had so many men to treat after the attack on Turin, in 1537, when he was in the service of King Francis I of France, that he ran out of oil. Next morning he was amazed to find that the men who had been left untreated were much better than those whose wounds had been scalded with oil, and thereafter he gave up this practice. Paré also gave a good account of the treatment of fractures and dislocations, and of herniotomy and other operations. Surgery in northern Europe was still largely in the hands of comparatively uneducated barbers and cutters, though some of these showed considerable skill. The itinerant lithotomist Pierre Franco, for example, was the first to perform suprapubic lithotomy for removing stone in the bladder. In Italy, surgery was in the hands of anatomists with a university training, like Vesalius and Hieronymo Fabrizio, and so it could benefit from the improvement of academic knowledge. The work in plastic surgery which had begun in the 15th century was carried on in the 16th century by the Bolognese Gaspere Tagliacozzi, who restored a lost nose by transplanting a flap of skin from the arm, leaving one end still attached to the arm until the graft on the nose had established itself.

While these anatomists and surgeons were extending the practical achievements of their predecessors, medical men of another group were endeavouring, as in other sciences, to return to antiquity. The first humanist doctors, such as Thomas Linacre (*c.* 1460–1524), physician to Henry VIII, tutor to Princess Mary and founder and first president of the College of Physicians, or Johannes Günther (1487–1574), who at Paris numbered Vesalius, Serveto and Rondelet among his pupils, were literary men rather than anatomists. They encouraged, and co-operated in making

the new Latin translations of Galen and Hippocrates which, along with the old, were printed in numerous editions from the end of the 15th century. They devoted their energy to establishing the text of these authors rather than to observation, and Mondino was objectionable to them not so much because he disagreed with nature as because he disagreed with Galen. They also began a violent attack on the old Latinised Arabic terminology of Mondino, which they 'purified' by substituting classical Latin or Greek for Arabic words and transformed into the anatomical terminology still in use.

It was in this atmosphere of both practical observation and humanist prejudice and literary research that the so-called father of modern anatomy, the Netherlander André Vésale of Brussels, or Andreas Vesalius (1514–64), began his work. In it he exhibits both features. The *De Humani Corporis Fabrica* (1543) may be regarded as the outcome of an attempt to restore both the letter and the standards of Galen. In it Vesalius followed Galen, as well as other authors to whom he did not acknowledge his debt, in many of their mistakes as well as in their true observations. He placed the lens in the middle of the eye, repeated Mondino's misunderstanding of the generative organs, represented the kidney as a sieve, and adduced some conclusions about human anatomy from the study of animals, a practice for which he criticised Galen. Further, he differed in no important respect from Galen in physiology. He shared his Greek master's eye for the exhibition of living function in anatomical structure. The function of an organ, according to Galen, was the final cause of its structure and mechanical action and thus the explanation of its presence. The inspiration of the anatomical research which he stimulated was strongly teleological, and Vesalius himself regarded the human body as the product of Divine craftsmanship. This must be accounted an important factor in the passion with which he pursued his dissections. But it was the illustrations that were the really revolutionary feature of *De Fabrica* (Pl. XXI). No anatomical drawings can compare with them except the unpublished ones by Leonardo himself; together they make the most brilliant

demonstration of how close the relations were between descriptive biology and naturalistic art. But the illustrations of *De Fabrica* achieve more than mere naturalism; the astonishing series representing the dissection of the muscles are at once an exhibition in detail of the relations between the structure and function of the muscles, tendons, bones and joints, and a dance of death, a drama played out by a corpse suspended from a gibbet against the background of a continuous landscape in the Euganean hills. Whose work the illustrations of *De Fabrica* and its companion volume the *Epitome* (published with it at Basel in 1543) were has not been finally determined, but it is practically certain that they emanated from the atelier of Titian, and that among the artists who worked on them under the supervision of the master was Vesalius himself.

The work of Vesalius contained by far the most detailed and extensive descriptions and illustrations yet published of all the systems and organs of the body. Though his account of the other organs usually does not compare with that of the bones and muscles, whose relations he illustrated very well, he nevertheless made a large number of new observations on veins, arteries and nerves, greatly extended the study of the brain though without entirely rejecting the *rete mirabile*, and showed that bristles could not be pushed through the supposed pores in the interventricular septum of the heart. He also repeated several of Galen's experiments on living animals and showed, for instance, that cutting the recurrent laryngeal nerve caused loss of voice. He showed that a nerve was not a hollow tube, though physiologists continued to believe the contrary until the 18th century. He showed also that an animal whose thoracic wall had been pierced could be kept alive by inflating the lungs with bellows.

A contemporary of Vesalius who, had his anatomical illustrations been published when they were completed in 1552, instead of in 1714, might have ranked with him as one of the founders of modern anatomy, was the Roman, Bartolomeo Eustachio (1520–74). He introduced the study of anatomical variations, particularly in the kidney, and gave excellent figures of the ear ossicles, the relations

of the bronchi and blood vessels in the lung, the sympathetic nervous system, the larynx, and the thoracic duct.

As events turned out, Vesalius, and not Eustachio, set his mark on anatomy. He made the centre of the subject Padua, where he was professor from 1537 until he became physician to the Emperor Charles V in 1544, and a large part of the subsequent history of anatomy down to Harvey is the story of Vesalius' pupils and successors. The first of these was his assistant Realdo Colombo (c. 1516–59), who experimentally demonstrated the pulmonary circulation of the blood (see above, p. 225). He was followed by Gabriel Fallopio (1523–62), who described the ovaries and the tubes called after him, the semi-circular canals of the ear, and several other structures. Fallopio's own pupils extended the Vesalian tradition at Padua into the study of comparative anatomy, but in the meantime similar interests had begun to develop elsewhere.

Many of those who were attracted by the printed editions of Pliny or of the Latin translations of Aristotle's zoological works developed from being humanist lexicographers into naturalists. A good example of this is William Turner (c. 1508–68) whose book on birds (1544), while being largely a compilation and accepting some legends such as that of the barnacle goose, also contained some fresh observations. Sixteenth-century zoology thus began as a gloss on the classics written increasingly from nature. The system of classification recognised by Albertus Magnus in Aristotle's writings, which the Oxford scholar and doctor, Edward Wotton, attempted to restore (1552), was the framework of the subject.

Besides birds, the first animals to attract attention were fish. Accounts of several local fish fauna, those of the sea at Rome and Marseilles and of the river Moselle, were written during the first half of the 16th century, but the scientific study of marine animals really began with the *De Aquatilibus* (1553) of the French naturalist, Pierre Belon (1517–64). Belon had already become well known for his account of a voyage to the eastern Mediterranean, during which he made some interesting biological observations (1533). He took an ecological view of his group; his

'aquatiles' were the fish of 'cooks and lexicographers' and included cephalopods and cetacea as well as *pisces*. He made the first modern contributions to comparative anatomy. He dissected and compared three cetacean types, realised that they breathed air with lungs, and compared the heart and skeleton to those of man. He depicted the porpoise attached by the umbilical cord to the placenta, and the dolphin with its new-born young still surrounded by foetal membranes. He also made a comparative study of fish anatomy, and in another small book, *Histoire Naturelle des Oiseaux* (1555), in which he intuitively recognised certain natural groups of birds, he depicted the skeletons of a bird and a man side by side to show the morphological correspondences between them (Pl. XXII). Another Frenchman, Guillaume Rondelet (1507–66), who became professor of anatomy at Montpellier and may have been 'the Physitian our honest Master Rondibilis' of Rabelais (who had also studied medicine there), included a similar heterogeneous collection of aquatic animals in his *Histoire Naturelle des Poissons* (1554–55). This was also a valuable work. In it he pointed out the anatomical differences between the respiratory, alimentary, vascular and genital systems of gill- and lung-breathing aquatic vertebrates, and depicted the viviparous dolphin and the ovoviviparous shark. He endeavoured to discover the morphological correspondence between the parts of the mammalian and piscine hearts. He discussed the comparative anatomy of gills, which he considered to be cooling organs, but he also showed that fish kept in a vessel without access to air would suffocate. He considered the teleostean swim-bladder, which he discovered, to be a kind of lung. Another heterogeneous work on aquatic animals published about the same time (1554), which is of interest in showing the influence of contemporary art in its excellent zoological illustrations, is that of H. Salviani (1514–72).

Another contemporary of these writers was the polyhistor and naturalist, Conrad Gesner. He attempted to draw up, on the lines of Albertus Magnus or Vincent of Beauvais, whom he quoted, an encyclopædia containing the observations of all his predecessors from Aristotle to Belon and

Rondelet. In the course of this he also made observations of his own and, through his vast correspondence, was a stimulus to others. In the zoological part of this work, the *Historia Animalium* (1551–58), he seems to have been so uncertain about classification that he arranged the animals in alphabetical order. In other works, containing extracts from the *Historia*, he set them out according to the Aristotelian system, omitting only the insects. The material for the insects, which had been compiled by Gesner, Wotton and Thomas Penny (*c.* 1530–88), was eventually published as Thomas Mouffet's *Theatrum Insectorum* (1634). Mouffet's 'insects' were those of Aristotle, and included myriapods, arachnids and various sorts of worms as well as the modern group of insects. His book contained a number of fresh observations, most of them the work of Penny. Gesner's work as an encyclopædist and zoologist was continued by Ulysses Aldrovandi (1522–1605), professor of natural history at Bologna, who among other things wrote the first book on fishes which did not include other aquatic forms.

Both Gesner and Aldrovandi included in their encyclopædic labours catalogues of fossils, or 'figured stones,' of which several collections had been made in the 16th century, including one by Pope Sixtus V at the Vatican. The fossils included in these collections were mainly echinoderms, mollusc shells and fish skeletons, and considerable interest attached to their origin. On this matter opinion in fact remained divided until the 18th century, and it was not easy to recognise the organic origin of some fossils. Those who held that fossils were not of organic origin explained them by such theories as astral influence or generation by subterranean vapours. Even among those who held that fossils were organic remains some believed them to have been transported to the mountains by the Flood. The theory that organisms had been fossilised where they had once lived and were found had persisted in the writings of Albertus Magnus. Girolamo Fracastoro (1483–1553) accepted this view and so did Agricola, who held that the process of mineral-formation and fossilisation was due to a *succus lapidescens*, which may have meant pre-

cipitation from solution. Another writer, the French potter
Bernard Palissy, who had learnt through Cardano of Leo-
nardo's ideas on these questions, went further and arrived
at some understanding of the significance of fossil forms
for comparative morphology. He regretted that Belon and
Rondelet had not described and drawn fossil fish as well
as living forms; they would then have shown what kind
of fish had lived in those regions at the time when the
stones in which they were found had congealed. He him-
self made a collection of fossils, recognised the identity of
a number of forms, such as sea-urchins and oysters, with
their living relatives, and even distinguished marine, lake
and river varieties. In contrast with these bold ideas, Ges-
ner admitted some fossils as petrified animals but regarded
others as products *sui generis* of the earth itself. He made
an attempt to classify them, taking their shape, the things
they resembled and so on, as his criteria. Aldrovandi re-
garded fossils not as the remains of normal living forms
but as incomplete animals in which spontaneous genera-
tion had failed full accomplishment.

Another aspect of biology which received fresh attention
during the 16th century was embryology, the study of
which was revived by Aldrovandi, who was inspired by Aris-
totle and Albertus Magnus to follow the development of
the chick by opening eggs at regular intervals. Into this he
initiated his Dutch pupil, Volcher Coiter, who, before fi-
nally settling at Nuremberg, studied also under Fallopio,
Eustachio and Rondelet. He was thus an intellectual de-
scendant of Vesalius, and the first of them to adopt the
comparative method. In the chick, on which his observa-
tions were on Aristotelian lines, he discovered the blasto-
derm, but he left it for Aldrovandi to explain how the
eggs passed from the ovary into the oviduct, and failed to
recognise that the avian ovary was homologous with the
mammalian 'female testis.' He made a systematic study of
the growth of the human foetal skeleton and pointed out
that bones were preceded by cartilages. He also made a
systematic study of the comparative anatomy of all verte-
brate types except the fishes. His emphasis on points of
difference, rather than homology, shows that he did not

fully grasp the significance of the comparative method, but his comparisons, beautifully illustrated by himself, greatly extended the range of the subject. He was most successful in his treatment of skeletons, of which he compared those of many different types, from frog to man. He also made a comparative study of living hearts. He tried to interpret the structure of the mammalian lung in terms of the simpler organs of frogs and lizards, and understood the difference in their respiratory mechanisms. He made a number of particular anatomical discoveries, of which that of the dorsal and ventral nerve roots was perhaps the most important, and he tried to classify mammals on an anatomical basis.

The comparative method was systematically extended to embryology by Fallopio's successor at Padua, Hieronymo Fabrizio, who was professor there at the same time as Galileo. Fabrizio made a number of contributions to anatomy. His embryological theory, like that of his pupil Harvey, was in principle entirely Aristotelian. But he held that the majority of animals were generated not spontaneously but from 'eggs,' gave good figures of the later stages of development of the chick (Pl. XXIIIA; cf. Pl. XXIIIB) and made a careful study of the embryology of a large number of vertebrates. In the last he paid particular attention to the foetal membranes and confirmed the assertion of Julius Cæsar Arantius (1564), that although the foetal and maternal vascular systems were brought into close contact with the placenta there was no free passage between them. He gave a clear account of other already known structures associated with the foetal blood system, such as the *ductus arteriosus* and the *foramen ovale* (discovered by Botallus, 1564). The valves in the veins had been observed by a number of anatomists, but Fabrizio published the first clear and adequate pictures of them (1603), which Harvey afterwards used to illustrate his book. In his comparative studies Fabrizio attempted to assess the points common to the various vertebrates and those defining specific differences. He held that each sense organ had its own special function and could perform no other, but although he drew the lens in its correct position in the eye he still believed

that it was the seat of vision. He attempted to analyse the mechanics of locomotion, and compared the actions of the internal skeleton of the vertebrate and the external skeleton of the arthropod. He observed that the worm moved by the alternate contraction of its longitudinal and circular muscles, and examined the relation of the centre of gravity to posture in the bird. It was not, however, until Borelli (1680) was able to make use of Galileo's mechanics that these problems received an adequate solution.

Fabrizio's comparative method was carried still further by his former servant and pupil, Giulio Casserio (1561–1616), who succeeded him at Padua. Casserio has been described as a great craftsman, who endeavoured to explain the fabric of man by reference to that of the lower animals. He divided his investigation, as Galen had done, into structure, action and uses (function). His method was first to describe the human condition in foetus and adult and then to follow it through a long series of other animals. This is well illustrated in his study of the organs of the voice and hearing, during which he described the sound-producing organs of the cicada and the auditory ossicles of a large number of land vertebrates, and discovered the internal ear of the pike (Pl. XXIV).

Casserio's successor, Adriaan van der Spieghel (1578–1625), whose chief work was to improve anatomical terminology, was the last of the great Paduan line, and after his time animal biology itself developed in a different direction. His contemporary at Pavia, Gasparo Aselli (1581–1626), discovered the lacteal vessels while dissecting a dog which had just had a meal containing fat. These are lymphatic vessels which conduct into the blood stream, at the jugular vein, fatty substances absorbed in the intestine, but Aselli thought they led from the intestine to the liver. Another contemporary, Marc Aurelio Severino (1580–1656), a pupil at Naples of the anti-Aristotelian philosopher Campanella, compiled on comparative anatomy a treatise entitled *Zootomia Democritaea* (1645) out of respect for his master's views. In this he recognised the unity of the vertebrates, including man, but he regarded man as the basic 'archetype,' determined by Divine design, and diver-

gences from this as due to differences in function. He discovered the heart of the higher crustacea, dissected but misunderstood that of cepholopods, recognised the respiratory function of fish gills, invented the method of studying blood vessels by injection with a solidifying medium, and recommended the use of the microscope. Though he wrote after Harvey, he suffered from the same defects as his predecessors.

The effort of the 16th-century anatomists had been to explore, describe and compare the structure of the human and the animal body, to make some attempt to relate the results by a zoological classification and to understand the variety of animal forms. They laid the foundation of work which was to lead to the theory of organic evolution, but, not only were their conceptions of physiology vague, inaccurate and uncoordinated, but also their inferences did not arise out of a critical and comprehensive consideration of the facts. Their conceptions of biological function were, in fact, largely inherited from the past, and as yet remained unrelated to their discoveries of structure. These matters were being brought into relation by another son of Padua, William Harvey (see above, p. 221 *et seq.*).

In embryology, Harvey made a number of advances. Although he has been criticised for his work on this subject, in fact he carried into this difficult field the same principles as he had used with such success in analysing the simpler problem of the movement of the blood. Among his positive contributions to comparative embryology were a number of particular observations on the placenta and other structures, the final recognition of the cicatricula on the yolk membrane as the point of origin of the chick embryo, and a clear discussion of growth and differentiation. Another contribution was implied by his remark in his *Exercitationes de Generatione Animalium* (1651), exercitatio 62: 'The egg is the common beginning for all animals.' Albertus Magnus, who had made a similar remark (see Vol. I, p. 154), certainly also accepted the spontaneous generation of the eggs or *ova* themselves; and since Harvey was not unequivocal on the point, especially in *De Motu Cordis*, opinions differ about whether he did too.

Some passages do definitely suggest that he held all plants and animals to originate from 'seeds' arising from parents of the same species, though these 'seeds' might sometimes be too small to be seen. As he declared in De Generatione Animalium: 'many animals, especially insects, arise and are propagated from elements and seeds so small as to be invisible (like atoms flying in the air), scattered and dispersed here and there by the winds; and yet these animals are supposed to have arisen spontaneously, or from decomposition, because their ova are nowhere to be seen.' Francesco Redi, who first experimentally disproved spontaneous generation in insects (1668), read Harvey's views in this sense. Thus, although Harvey did not understand the nature of the ovum, which he still identified in insects with the larva or pupa, and in mammals, with small embryos surrounded by their membrane or chorion, his ideas, which crystallised into the omne vivum ex ovo that appeared on the frontispiece of his book, stimulated research into the subject by his followers.

Harvey's own observations led him to reject both the Aristotelian and Galenic theories of fertilisation. According to Aristotle, the uterus of a fertilised female should have contained semen and blood, according to Galen a mixture of male and female semen. In the king's deer, which Harvey dissected at Hampton Court, he could find no such visible proof of conception for some months after mating. He was unfortunate, because in this respect deer are peculiar; but he could also see nothing for several days in more normal animals such as dogs and rabbits. He therefore concluded that the male contributed an immaterial influence like that of the stars or of a magnet, which set the female egg developing. Although the production of eggs in ovarian follicles was not discovered until after Harvey, he may thus be considered the originator of the 17th-century 'ovist' theory according to which the female contributed the whole of the embryo. After Leeuwenhoek, with his microscope, had discovered the spermatozoon (1677), the opposite school of 'animalculists' made the same claim for the male, and the resulting controversy continued throughout most of the 18th century.

The other great embryological controversy over which Harvey's followers spent their energies was that of epigenesis and preformation. Harvey himself had clearly reaffirmed Aristotle's preference for the former, at least in sanguineous animals; he held development to be the production of structures *de novo* as the embryo approached the final adult form. Against this later ovists and animalculists alike held that the adult was formed by the 'evolution,' or unfolding, of parts already completely present in the germ. This was more in keeping with the mechanism of the age, and the year after Harvey's death Gassendi published a theory of panspermatic preformationism based on his atomic theory. Some time earlier, an even completer mechanistic theory of biology had been worked out by Descartes (see above, p. 238 *et seq.*).

This work on reproduction was to lead to the formulation of the germ theory of disease, though that was not fully understood until the time of Pasteur in the 19th century. In the early 16th century a theory that diseases were caused by the transference of *seminaria*, or seeds, was put forward by Fracastoro. He is famous for introducing the name syphilis and for describing that disease, which had first appeared in a virulent form in 1495 in Naples, then occupied by Spanish troops, during the siege by the soldiers of Charles VIII of France. He set forth his theory of disease in his *De Contagione*, published in 1546, in which he reiterated the already known facts that disease could be transmitted by direct contact, by clothing and utensils, and by infection at a distance as with smallpox or plague (see Vol. I, pp. 228–30). To explain such action at a distance, he made use of a modification of the old theory of the 'multiplication of species'; he said that during the putrefaction associated with disease minute particles of contagion were given off by exhalation and evaporation, and that these 'propagated their like' through the air or water or other media. When they entered another body, they spread through it and caused the putrefaction of that one of the four humours to which they had the closest analogy. To such *seminaria* Fracastoro attributed the spread of contagious phthisis, rabies and syphilis.

Fracastoro seems also to have been the first to recognise typhus, and the habit of carefully recording case-histories, which has been seen in the *consilia* and plague tracts made since the 13th century, produced a number of good accounts of diseases in the 16th century, for example the clear description of the sweating sickness published by John Caius in 1552. This practice increased in the 17th century and produced such excellent clinical records as Francis Glisson's account of infantile rickets in 1650, Sir Theodore Turquet of Mayerne's medical history of King James 1, and the careful descriptions of measles, gout, malaria, syphilis, hysteria, and other diseases made by Thomas Sydenham (1624-89). This insistence on observation, and suspicion of the all-too-facile theories which had prevented new approaches to the facts, led to a great increase in empirical knowledge and in empirical methods of treatment; indeed even now, in the 20th century, medicine is still largely an empirical art. As early as the early 16th century, if not still earlier, mercury was used for syphilis, and from the early 17th century cinchona bark, the source of quinine, was used for malaria. This had been introduced into Europe from Peru by Jesuit missionaries after whom it became known as 'Jesuits' bark.' A clear understanding of infectious diseases, as indeed the understanding of the causes of the functional and organic disorders of the body, had to await the gradual acquisition of the fundamental knowledge of biology and physiology during the 18th and 19th centuries.

(8)   PHILOSOPHY OF SCIENCE AND CONCEPT OF NATURE IN THE SCIENTIFIC REVOLUTION

By the middle of the 17th century European science had gone a long way since Adelard of Bath had demanded explanations in terms of natural causation, and since the experimental and mathematical methods had begun to develop within the predominantly Aristotelian system of scientific thought of the 13th and 14th centuries. Certainly

in experimental and mathematical technique revolutionary progress had been made by the 17th century, and this was to go on with breathtaking speed throughout that century. To take only one science as an example, astronomy in 1600 was Copernican, and not even completely so; in 1700 it was Newtonian, and was supported by the impressive structure of Newtonian mechanics. Yet the statements on aims and methods expressed by the spokesmen of the new 17th-century science were remarkably similar to those expressed by their predecessors in the 13th and 14th centuries, who were, in fact, also spokesmen of modern science at an earlier stage in its history. They were remarkably similar—with a difference.

The utilitarian ideal, for example, was given expression by Francis Bacon in words very like to those of his 13th-century namesake, even down to the particular value he placed on the inductive method. 'I am labouring to lay the foundation,' said Bacon in the preface to his *Great Instauration*, 'not of any sect or doctrine, but of human utility and power.' The purpose of science was to gain power over nature. The object of the Great Instauration, or new method, was to show how to win back that dominion which had been lost at the Fall. In the past, science had been static, while the mechanical arts had progressed, because in science observation had been neglected. It was only through observation that knowledge of nature could be gained; it was only knowledge that led to power; and the knowledge that the natural scientist was to look for was knowledge of the 'form,' or causal essence, whose activity produced the effects observed. Knowledge of the form gave mastery over it and its properties, and so the positive task of Bacon's new method was to show how to obtain knowledge of the form. As he declared in the *Novum Organum* (1620), book 1, aphorism 3: 'Human knowledge and human power are one; for where the cause is not known the effect cannot be produced. Nature to be commanded must be obeyed; and that which in contemplation is as the cause is in operation as the rule.' What he meant by the 'form' of a body or a phenomenon he

explained further in book 2, aphorisms: 'For though in nature nothing really exists besides individual bodies, performing pure individual acts according to a fixed law, yet in philosophy this very law, and the investigation, discovery, and explanation of it, is the foundation of knowledge as well as of operation. And it is this law, with its clauses, that I mean when I speak of Forms; a name which I rather adopt because it has grown into use and became familiar.'

The parenthetical conclusion to this quotation is a warning that Bacon may be concealing in his deceptively scholastic language concepts far removed from the 'substantial form' and real qualities in the sense of the scholastic 'natures.' It also serves as a reminder that the historian of scientific method must necessarily include in his field of consideration not only the logical procedures described and used by a natural philosopher, but also—and without these he will understand nothing—the actual problems to which the procedures were applied and the assumptions made concerning the kind of explanation they should yield. For example it is impossible to see the point of Grosseteste's or Ockham's discussions of scientific method without the context of the philosophy of nature to which they applied. Galileo and Kepler aimed their analyses of scientific method at the particular kinematic and dynamical problems they were trying to solve; their point can be seen only in relation to these, and to the kinds of laws they expected to discover.

The procedures of science are methods of answering questions about phenomena; the questions give definition to the phenomena and constitute them into problems. Much of what is asked about such data will be determined simply by the technical procedures, mathematical and experimental, in current use or being developed. But the form the questions take, the direction and extent to which they are pressed in the search for an explanation, will inevitably be strongly influenced by the investigator's philosophy or conception of nature, his metaphysical presuppositions or 'regulative beliefs,' for it is these that will determine his conception of the real subject of his inquiry, of the direction in which the truth hidden in the appearances will be

found. It is these that will often determine what a scientist regards as significant in a problem; they may inspire his scientific imagination, as they did with Kepler and Galileo; and they may set limits to what he regards as admissible in an explanation, as the objection to action at a distance did for the critics of Newton's theory of gravitation. These philosophical assumptions may of course themselves be profoundly modified in the course of a scientific investigation. They may be falsified by observation, as Newton falsified the assumption of the circularity of all celestial motion. Or they may be in themselves not empirically falsifiable, like the scholastic conception of 'natures' or the belief that all phenomena can be reduced to matter and motion. Such conceptions are abandoned or modified only by re-thinking. But there has never been natural science with no preconception at all of theoretical objectives of a philosophical kind.

In the actual history of science many of the most fruitful theories have been developed from preconceived ideas of the kinds of laws or theoretical entities that will be discovered to explain the phenomena. The history of the inquiry has to a large extent consisted of using the sharp tools of mathematics and experiment to carve out of these preconceptions a theory exactly fitting the data. A good example of this is the atomic theory, first seen as scientific material of this kind in the 17th century and eventually reduced to exact empirical form by John Dalton in 1808. So far as scientific method is concerned, the whole period from the 13th to the 17th century can be seen as one in which the functions both of the experimental principles of verification and falsification and correlation, and of mathematical techniques, were understood and applied with increasing effect to reduce philosophies of nature to exact science (cf. above, p. 10 *et seq.*). For example the Neoplatonic philosophy of nature, with its geometrical conception of the ultimate 'form' of things, first became scientifically significant with Grosseteste's philosophy of light. But in spite of his analysis of the logic of experimental science Grosseteste himself was capable of leaving the explanations he derived from his Neoplatonism not only very

loosely connected with the data but sometimes actually contradicting them. It was the more technical and less philosophical mathematical and experimental investigators of the period, inspired by Euclid and Archimedes more than Plato and Aristotle, who were more empirically accurate in practice; and it was only when the technical procedures were fully exploited by Galileo and Kepler that Neoplatonism yielded exact science.

It was precisely in such a critical role that Francis Bacon conceived his inductive method for 'the discovery of forms.' By 'form' Bacon meant something quite specific: geometrical structure and motion. The common idea of him as a pure empiricist, starting with no preconceived ideas or hypotheses, is by no means borne out by his principal work on scientific method, the *Novum Organum*, although it is nearer the mark in the endless tables of instances forming the 'Natural and Experimental Histories' of the *Sylva Sylvarum*. Bacon's achievements are those of a philosopher with a clear grasp of the function of the empirical principle but almost none at all of the technical procedures necessary, not only to solve problems, but even to formulate them in a scientifically significant manner.

In his *Novum Organum* Bacon of course explicitly set out to replace the *Organum* of Aristotle, but when compared with the various conceptions of scientific method held in classical and early modern times it is clear that Bacon's method has far more in common with Aristotle's than do, for example, the postulational methods of Archimedes and Galileo. He based his method on the analysis of matter rather than the idealisations of mechanics; it was aimed at discovering the composition of bodies, and it is significant that a large number of his examples were taken from chemistry. But if one is looking for the ancestry of his method, it is easy to see it in the postulational method of Democritus and in Plato's dialectic (cf. above, pp. 8, 139–40).

The current view against which Bacon and other contemporary advocates of the 'new philosophy' were writing was that the explanation of phenomena could be given in terms of the qualitative substantial forms and real quali-

ties forming the 'natures' of the scholastics. Finding these unhelpful, the natural philosophers of the period assimilated their philosophy of nature to the new science by developing a more mathematical conception of the 'form' based on the atomism of Democritus and Epicurus and of Hero of Alexandria (see Vol. I, p. 28, note 4; above, p. 37, note 7), while Galileo and Kepler came to distinguish between the primary, real, geometrical qualities actually belonging to bodies and the secondary, subjective qualities produced by the action of these on the organs of sense (see below, p. 302). Bacon was one of the earliest modern writers to propose the complete reduction of all events to matter and motion. In his *Cogitationes de Natura Rerum* he had written: 'The doctrine of Democritus concerning atoms is either true, or useful for demonstration.' His proposal for 'the discovery of forms' in the *Advancement of Learning* (1605) was an inquiry into the explanation of the properties of bodies, but he asserted that this had got too far from experiment. His object was to base the inquiry not on the atoms of the philosophers but on induction. Then, as he said in the *Novum Organum*, book 2, aphorism 8, 'We shall be led only to real particles, such as really exist.' These constituted the 'latent configuration' of the form, hidden from sight but discoverable by inductive reasoning. Their movement constituted the 'latent process,' variation in motion producing different manifest effects in the 'nature,' by which he meant any type of observable occurrence, such as heat, light, magnetism, planetary motion, fermentation. Thus his preconception of the kind of entities his inductive analysis would yield was just as definite as that of the scholastic writers on scientific method who discussed the 'resolution' of bodies into the four Aristotelian elements and causes or of a disease into one of a set of preconceived species of a genus (cf. above, pp. 14, 25–28). And Bacon described the form, as he conceived it, in language similar to that used by the scholastics of the four Aristotelian causes, the conditions necessary and sufficient to produce the observed effect. 'For,' he said in book 2, aphorism 4, 'the Form of a nature is such, that given the Form the nature infallibly follows.' This led him

to base the inquiry for the form on the methods of agreement or presence, difference or absence, and concomitant variation (cf. above, p. 137).

Bacon's method followed the pattern of the inductive and deductive processes already seen in his medieval predecessors. His chief contribution to the theory of induction was to set out very clearly and in great detail both the method of reaching the definition of a 'common nature,' or form, by collecting and comparing instances of its supposed effects, and the method of eliminating false forms (or what would now be called hypotheses) by what he called 'exclusion.' This was analogous to Grosseteste's method of 'falsification' (*falsificatio*). Bacon said in the *Novum Organum*, book 1, aphorism 95:

> Those who have handled sciences have been either men of experiment or men of dogmas. The men of experiment are like the ant; they only collect and use: the reasoners resemble spiders, who make cobwebs out of their own substance. But the bee takes a middle course, it gathers its material from the flowers of the garden and of the field, but transforms and digests it by a power of its own. Not unlike this is the true business of philosophy; for it neither relies solely or chiefly on the powers of the mind, nor does it take the matter which it gathers from natural history and mechanical experiments and lay it up in the memory whole, as it finds it; but lays it up in the understanding altered and digested. Therefore from a closer and purer league between these two faculties, the experimental and the rational (such as has never yet been made) much may be hoped. . . . Now [he went on in book 2, aphorism 10] my directions for the interpretation of nature embrace two generic divisions; the one how to educe and form axioms from experience; the other how to deduce and derive new experiments from axioms.

The first step towards the discovery of a form was to make a purely empirical collection of instances of the phenomenon or 'nature' being investigated. As an illustration of both his method and the kinds of things that should

be investigated, he gave his well-known example of the 'form of heat.' As he said in the *Novum Organum,* book 2, aphorism 10: 'We must prepare a *Natural and Experimental History.'* The next step was made by what he claimed to be a new kind of induction, hitherto used in part only by Plato. The current kind of induction 'by simple enumeration' was, he said in book 1, aphorism 105, generally based on too few facts and 'exposed to peril from a contradictory instance. . . . But the induction which is to be available for the discovery and demonstration of sciences and arts, must analyse nature by proper rejections and exclusions; and then, after a sufficient number of negatives, come to a conclusion on the affirmative instances.' To make this 'true and legitimate' induction the observations must be classified into three 'Tables and Arrangements of Instances.' The first was a table of 'Essence and Presence' or agreement, which included all events where the form sought (e.g., heat) was present; the second was a table of 'Deviation or of Absence in Proximity' which included all events where the effects of the form sought were not observed; the third was a table of 'Degrees or Comparison' which included instances of variations in the observed effects of the form sought either in the same or in different subjects. Induction then consisted simply of the inspection of these tables. "The problem is,' said Bacon in the *Novum Organum,* book 2, aphorisms 15 and 16,

> upon a review of the instances, all and each, to find such a nature as is always present or absent with the given nature, and always increases and decreases with it . . . The first work therefore of true induction (as far as regards the discovery of Forms) is the rejection or exclusion of the several natures which are not found in some instance where the given nature is present, or are found in some instance where the given nature is absent, or are found to increase in some instance when the given nature decreases, or to decrease when the given nature increases. Then indeed after the rejection and exclusion had been duly made, there will remain at the bottom,

all light opinions vanishing into smoke, a Form affirma-
tive, solid and true and well defined.

On the basis of this uncliminated residue the investi-
gator then embarked upon what he called in aphorism 20
'an essay of the Interpretation of Nature in the affirmative
way.' The first stage in this process led only to the 'First
Vintage' or a working hypothesis. So, he concluded: 'From
a survey of the instances, all and each, the nature of which
Heat is a particular case appears to be Motion . . . Heat
itself, its essence and quiddity, is Motion and nothing
else.' From this hypothesis new consequences were de-
duced and tested by further observations and experiments
until eventually, by repeated and varied observation fol-
lowed by climination, the 'true definition' of the form was
discovered, and this gave certain knowledge of the reality
behind the observed effects, knowledge of the true law in
all its clauses. 'The Form of a thing,' he said in *Novum
Organum*, book 2, aphorism 13, 'is the very thing itself,
and the thing differs from the form no otherwise than as
the apparent differs from the real, or the external from
the internal, or the thing in reference to man from the
thing in reference to the universe.'
The form for Bacon was always some mechanical disposi-
tion: induction eliminated the qualitative and the sensible
leaving geometrical fine structure and motion. The form of
heat was thus motion of particles; the form of colours a
geometrical disposition of lines. In fact, by Bacon's time
the word 'nature' itself had come to mean mechanical prop-
erties, the *natura naturata* of the Renaissance. The spon-
taneous animating principle, *natura naturans*, of such
writers as Leonardo da Vinci or Bernardino Telesio (1508–
88) had practically disappeared.
The discovery of the form was the end of the 'experi-
ments of Light' which occupied the essential first stage in
science but, as Bacon put it in the *Great Instauration*:

those twin objects, human Knowledge and human
Power, do really meet in one; and it is from ignorance of
causes that operation fails.

The final purpose of science was power over nature. More-over, he said in the *Novum Organum*, book 1, aphorisms 73 and 124:

> fruits and works are as it were sponsors and sureties for the truth of philosophies . . . Truth therefore and utility are here the very same things: and works themselves are of greater value as pledges of truth than as contributing to the comforts of life.

Thus, when Bacon excluded final causes from science it was not because he did not believe in them, but because he could not imagine an applied teleology as there was an applied physics. By following his 'experimental philosophy' he held that future humanity would achieve an enormous increase in power and material progress. As he expressed it in the *Novum Organum*, book 1, aphorism 109:

> There is therefore much ground for hoping that there are still laid up in the womb of nature many secrets of excellent use, having no affinity or parallelism with any-thing that is now known, but lying entirely out of the beat of the imagination, which have not yet been found out.

And he believed that the final achievement of the branch of science which he described in the *Advancement of Learning* as 'Natural Magic' would be the transmutation of the elements.

It was through his utilitarianism and his empiricism rather than the actual canons of his inductive method that Bacon chiefly influenced his followers, although his ideas on method certainly had some effect in England. Even Harvey declared in his *De Generatione*, exercitatio 25: 'in the words of the learned Lord Verulam to "enter upon our second vintage". . .' His most important influence was in the Royal Society. Bacon's description of the research institute, Solomon's House, in his *New Atlantis*, published posthumously in 1627, was the real inspiration of the vari-ous schemes for scientific institutions or colleges that were finally realised in the foundation of the Royal Society. Un-der Bacon's influence the Fellows dedicated themselves

from the beginning to experimental inquiries, and they aimed at promoting not only 'Natural Knowledge' but knowledge that would be useful in trades and industries. In the *Advancement of Learning* Bacon declared the true end of scientific activity to be the 'glory of the Creator and the relief of man's estate.' Echoing this, the second charter of the Royal Society, which received the Great Seal on 22 April 1663 and by which the Society is still governed, laid down that the investigations of its Fellows 'are to be applied to further promoting by the authority of experiments the sciences of natural things and of useful arts, to the Glory of God the Creator, and the advantage of the human race.' The Fellows were asked by the English government to investigate such problems as the practices used in navigation and in mining, and they themselves saw in technology a means of improving the empirical basis of science (cf. above, pp. 123–24). It was this emphasis on the usefulness of science, as well as his empiricism, that made Bacon the hero of d'Alembert and the French encyclopædists of the 18th century.

Thomas Sprat in his *History of the Royal Society* (1667) expressed a typical opinion of Bacon in describing his writings as the best 'defence of Experimental Philosophy, and the best Directions, that are needful to promote it,' and in saying at the same time that Bacon's Natural Histories were not only sometimes inaccurate but also that he seemed 'rather to take all that comes, than to choose, and to heap, rather than to register.' A typical example is the inquiry into the form of heat, where the instances ranged from warm feathers to the sun's rays, and from 'hot' pepper to the 'burning' of the hands by snow. Bacon's influence certainly sometimes led to a blind empiricism, but more typical was that on a man like Robert Hooke, who was one of those who actually made use of Bacon's methods, expounding them in his *General Scheme* published in the *Posthumous Works* (1705), but he was too good an experimentalist, mathematician, and deviser of hypotheses to be in any way restricted by what Bacon had laid down.

The only scientist of the period who saw himself as a complete Baconian was Boyle: 'designed by Nature to suc-

ceed' to the fame of the great Verulam as the *Spectator* described him in 1712. 'By innumerable experiments He, in great Measure, filled up those Plans and Out-Lines of Science, which his Predecessor had sketched out.' Boyle was extremely influential in handing on Bacon's empiricism, his distaste for systems, his insistence on the primacy of experiment over theory, to Newton himself and to the 18th century. For example the significant *Prœmial Essay* in his *Physiological Essays* (1661) was aimed at reinforcing Baconian empiricism as against Cartesian rationalism and the speculative development of systems far beyond the experimental evidence. As he wrote: 'It has long seemed to me none of the least impediments of the real advancement of true natural philosophy, that men have been so forward to write systems of it, and have thought themselves obliged either to be altogether silent, or not to write less than an entire body of physiology.' But Boyle's work and contemporary reputation are revealing just because they show the influence of the side of Bacon that has so often been neglected: his philosophy of nature. No more than Bacon was Boyle a completely anti-theoretical experimentalist; he is more truly seen, as his 18th-century editor Peter Shaw described him, as the 'restorer of the mechanical philosophy' in England.[22] As he himself wrote in the *Producibleness of Chymical Principles* (1679) appended to the second edition of the *Sceptical Chymist*: 'For though sometimes I have had occasion to discourse like a Sceptick, yet I am far from being one of that sect; which I take to have been little less prejudicial to natural philosophy, than to divinity itself.'

In fact, far from being a sceptical empiricist, Boyle was very ready to make use of hypotheses as aids to research. Arguing in favour of the 'Corpuscularian doctrine' in the preface of his *Mechanical Origin . . . of . . . Qualities* (1675), he wrote: 'For, the use of an hypothesis being to render an intelligible account of the causes of the effects, or phænomena proposed, without crossing the laws of nature, or other phænomena; the more numerous and the more

various the particles are, whereof some are explicable by
the assigned hypothesis, and some are agreeable to it, or, at
least, are not dissonant from it, the more valuable is the
hypothesis, and the more likely to be true. For it is much
more difficult to find an hypothesis that is not true, which
will suit with many phænomena, especially if they be of
various kinds, than but with a few.' But he concluded: 'I
intend not therefore by proposing the theories and con-
jectures ventured at in the following papers, to debar my-
self of the liberty either of altering them, or of substituting
others in their places, in case a further progress in the
history of qualities shall suggest better hypotheses or ex-
plications.' In an unfinished and unpublished tract en-
titled *Requisites of a Good Hypothesis*, he made a further
distinction between a 'good hypothesis,' which explained
the largest number of facts without contradiction, and an
'excellent hypothesis,' which was the unique explanation,
or, at least, was uniquely good. Such an hypothesis must
not only yield predictions, but such predictions as will en-
able it to be put to experimental test. The fragment is
worth quoting as a whole:

The Requisites of a good Hypothesis are:
    That it be Intelligible.
    That it neither Assume nor Suppose anything Impos-
sible, unintelligible, or demonstrably False.
    That it be consistent with itself.
    That it be fit and sufficient to Explicate the *Phæ-
nomena*, especially the chief.
    That it be, at least consistent, with the rest of the
Phænomena it particularly relates to, and do not con-
tradict any other known *Phænomena* of nature, or mani-
fest Physical Truth
The Qualities and Conditions of an *Excellent Hypothe-
sis* are:
    That it be not *Precarious*, but have sufficient Grounds
in the nature of the Thing itself or at least be well rec-
ommended by some Auxiliary Proofs.
    That it be the *Simplest* of all the good ones we are able

to frame, at least containing nothing that is superfluous or Impertinent.

That it be the *only* Hypothesis that can Explicate the Phænomena; or at least, that do[e]s Explicate them so well.

That it enable a skilful Naturalist to foretell future Phænomena by their Congruity· or Incongruity to it; and especially the events of such Experim'ts as are aptly devis'd to examine it, as Things that ought, or ought not, to be consequent to it.[23]

Boyle's problem was the same as Bacon's and of other contemporaries faced with the scientific uselessness of the Aristotelian doctrine of 'natures.' As he wrote in the preface of the *Mechanical Origin . . . of . . . Qualities:* 'if, by a bare mechanical change of the internal disposition and structure of a body, a permanent quality, confessed to flow from its substantial form, or inward principle, be abolished, and, perhaps, also immediately succeeded by a new quality mechanically producible; if, I say, this come to pass in a body inanimate, especially, if it be also, as to sense similar, such a phænomenon will not a little favour that hypothesis, which teaches, that these qualities depend upon certain contextures, and other mechanical affections of the small parts of the bodies, that are endowed with them, and consequently may be abolished when that necessary modification is destroyed.' The diverse and prolix collection of essays that form the product of his forty years' devotion to natural philosophy had a single aim: to discover through experiment an explanation of the properties of bodies, to develop a universal theory of matter on the same intelligible principles as the new science of mechanics. By his analysis of 'the origin of forms and qualities' Boyle meant just what Bacon meant by 'the discovery of forms.' The object of his 'corpuscular philosophy,' neither atomist nor Carte-

sian but developed along the lines suggested by Bacon, was
to explain all the manifest properties of bodies by the two
principles of matter and motion, by the size, shape and
motion of particles as indicated by extensive experiments.
This form of the mechanical philosophy was reinforced by
Boyle's experimental production of a vacuum and his ex-
periments on the air. The strongly empirical aspect of his
thinking is shown for example by his unwillingness to com-
mit himself as to the *cause* of the air's elasticity, of which
he stated the quantitative characteristics in 'Boyle's Law.'
There is a parallel to this in the attitude taken by Edme
Mariotte, who also formulated this law, and by Pascal.
Boyle was always only too careful to test and illustrate by
experiment the many particular hypotheses he formed in
the course of his researches. But the form of these particu-
lar hypotheses and the kind of theoretical entities they
contained was determined by a philosophy of nature that
was not submitted to falsification but was a 'regulative
belief' *assumed* in all his scientific thinking. This was the
belief in universal mechanism which was held by Bacon no
less than by Descartes and was soon to become predictively
fruitful in the world-machine of Newton. As Boyle wrote in
his *Excellency and Grounds of the Mechanical Hypothesis*
(1674): 'By this very thing that the mechanical principles
are so universal, and therefore applicable to so many things,
they are rather fitted to include, than necessitated to ex-
clude, any other hypothesis, that is founded in nature, as
far as it is so.'

The desire for certain knowledge of nature, which in-
spired Francis Bacon's work on method, and which in fact
since St. Augustine or indeed since Plato had inspired the
whole rationalist tradition of European thought, with its
belief that what is certain is true of reality, was the princi-
pal motive behind all 17th-century science; it is what made
the 17th century so conscious of method. Until the end of
the 17th century, when this Aristotelian form of predica-
tion of attributes as inhering in real persisting substances
began to be criticised in the new empiricism of John Locke
(1632–1704), all scientists were inspired by the faith that
they were discovering through and behind the particular

observed phenomena the intelligible structure of the real world. And so it was supremely important to have a method that would facilitate this discovery of real nature behind the appearances and that would guarantee the certainty of the result. The same emphasis on method is seen in all science, whether in the numerous 'methods' put forward by botanists in search of a 'natural' as opposed to a merely artificial system of classification, or in the experimental method and the mathematical method of chemists and physicists.

Except for some biologists to whom organisms still presented a problem, by the middle of the 17th century the assumption made by nearly every natural philosopher who set out to discover this real physical world was that what they would discover would be something mathematical in form. It was Galileo who laid down the methodological desiderata for this mechanical philosophy by his explicitly kinematic treatment of motion and firm rejection of any consideration of Aristotelian 'natures' and causes, for example in the *Two New Sciences* (see above, pp. 146–48; cf. p. 86 *et seq.*). He described the concept of nature which his methods had in view very clearly in 1623 in *Il Saggiatore*, both in question 6 (see above, p. 142) and in his famous distinction between primary and secondary qualities in question 48. Discussing Aristotle's remark in *De Cælo* (book 2, chapter 7) that 'motion is the cause of heat,' he wrote:

> But first I want to propose some examination of that which we call heat, whose generally accepted notion comes very far from the truth if my serious doubts be correct, in as much as it is supposed to be a true accident, affection, and quality really residing in the thing which we perceive to be heated. No sooner do I form a conception of a piece of matter or a corporeal substance, than I feel the need of conceiving that it has boundaries which give it this or that shape; that relative to others it is large or small; that it is in this or that place, in this or that time; that it is moving or still; that it touches or does not touch another body; that it is single, few, or

many; nor can I, by any effort of imagination, dissociate it from those qualities (*condizioni*). But I feel no need to apprehend it as necessarily accompanied by such conditions as to be white or red, bitter or sweet, sounding or silent, pleasant or evil smelling. On the contrary, if the senses had not perceived these qualities, perhaps the reason and imagination alone would never have arrived at them. Therefore I hold that these tastes, odours, colours, etc., on the part of the object in which they seem to reside, are nothing more than pure names, and exist only in the sensitive body, so that if the animate being (*animale*) were removed, these qualities would themselves vanish. But yet, having given them special names different from those of the other primary and real qualities (*accidenti*), we would persuade ourselves that they also exist just as truly and really as the latter. I can explain my conception more clearly with an example. I pass a hand, first over a marble statue, then over a living man. As to the hand's own action, this is the same with respect to both bodies—that is, the primary qualities, motion and touch, for we call them by no other names. But the animate body which suffers such operations feels different sensations (*affezioni*) according to the different parts touched. For example, when touched under the soles of the feet, on the kneecaps, or under the armpits, it feels, besides the common feeling of being touched, another to which we have given a particular name, calling it tickling. This feeling is all ours, and does not belong to the hand at all; and it seems to me that it would be a grave mistake to say that, besides motion and touch, the hand has in itself another faculty, different from these, namely the tickling faculty, so that tickling would be a quality residing in the hand. A small piece of paper, or a feather, lightly drawn over any part of our body you wish performs, in itself, the same action everywhere, that is it moves and touches; but in us, touching between the eyes, on the nose, or under the nostrils, it excites an almost unbearable tickling, though in other parts we can hardly feel it at all. Now this tickling is all in us, and not in the feather, and if the animate and sensitive body

were removed, it would be no more than a mere name (*un puro nome*). I believe that many qualities (*qualità*) which are attributed to natural bodies, such as tastes, odours, colours, and others, have a similar but no greater existence.

He went on to relate each of four senses to the four traditional elements, in a corpuscular theory of matter. Touch corresponded to earth, taste to water, smell to fire, hearing to air. The fifth sense, vision, corresponded to light, ether. Thus earth was continually being resolved into 'minimal particles' (*particelle minime*) of different kinds. Some of these, having been 'lodged on the upper surface of the tongue, and penetrating its tissue after being dissolved in its moisture, produce tastes that are pleasant or unpleasant according to the diversity of contact provided by the different shapes of these particles, and according to whether they are few or many and more or less rapidly in motion.' Similarly for smell and hearing. 'But,' he concluded, 'I hold that there exists nothing in external bodies for exciting in us tastes, odours and sounds other than sizes, shapes, numbers, and slow or swift motions; and I conclude that if the ears, tongue and nose were removed, shape, number and motion would remain but there would be no odours, tastes or sounds, which apart from living beings I believe to be nothing but names, exactly as tickling is nothing but a name if the armpit and the skin inside the nose be removed.' As to the relation of vision to light, he concluded: 'Of this sensation and the things connected with it I do not pretend to understand more than very little, and since I have not much time to explain, or rather to sketch that little, I shall remain silent.'

In this famous passage Galileo outlined a true mechanical philosophy of nature. Combining Democritus' distinction between the perceptual world of sensory appearances (which Aristotle took to be real) and the conceptual real world of the primary qualities, with a corpuscular conception of matter derived from Hero of Alexandria (see Vol. I, p. 28, note 4; above, p. 37, note 7), he offered an explanation of the manifest physical properties of bodies in

terms of the characteristics of their component particles. These moreover he conceived dynamically, taking into account the variation of their motion, and seeming to envisage the extension to the particles of mathematical laws such as had proved so successful in dealing with the motions of macroscopic bodies.

Galileo's ultimate scientific aim of discovering the real structure of the physical world, of reading the real book of nature in mathematical language, is clearly shown not only in his controversies over the Copernican theory but in everything he wrote about the philosophy of science (see above, pp. 135 *et seq.*, 200 *et seq.*). Certainly this envisaged the establishing of a quantitative and empirically-verified connection between the real but unobservable entities defined by the primary qualities and the observed properties of which these entities were the cause. Galileo himself also provided, in his 'resolutive-compositive' method, the effective means of exploring and establishing such a connection. But the tactics exemplified in his kinematic approach to motion, his method of breaking up a problem into separate questions and proceeding step by step, meant that Galileo himself never in fact developed his mechanical philosophy into a scientific explanation, a theory deductively related to the prediction of the data. In fact with the current state of scientific knowledge it would have been rash speculation to attempt such a development systematically. Galileo preferred to keep it as the ultimate goal of his empirical progress.

It was Descartes who first not only claimed that the mechanical philosophy was the universal explanation of all physical phenomena, but also attempted to carry out the explanations in detail. Lacking Galileo's scientific finesse and sense of empirical fact, Descartes criticised Galileo's treatment of motion for providing mathematical descriptions without philosophical basis and therefore without explanation (see above, p. 162). Descartes' confident philosophical rationalism, his clear conception of a universal philosophy of nature as the goal of science, swept him into regions of speculation before which much better scientists hesitated. But just this speculative rashness was the

source of his uniquely important contribution to the scientific movement. His bold unifying conception of the universe as an integrated whole explicable by universal mechanical principles applicable equally to organisms and to dead matter, to the microscopic particles and to the heavenly bodies, provided the succeeding generations of natural philosophers—astronomers, physicists, chemists, physiologists—with a programme. He gave them an hypothesis, a model whose properties they could exploit. Becoming the prevailing philosophy of nature by the mid-17th century, Cartesianism also brought out into the open philosophical problems inherent in the mechanical philosophy regarded as the whole truth and nothing but the truth. Even when Descartes' epistemology and metaphysics were rejected, his physics had a dominant influence, in the Royal Society as much as in the Académie des Sciences. Any new system had to make its way against it, and even the most celebrated alternative, the Newtonian system, to which Cartesian resistance in France was overcome only by Maupertuis (1698–1759) and Voltaire (1694–1778), was based on the same general programme of discovering the unifying laws of cosmology. It succeeded by establishing this Cartesian objective with greatly superior empirical precision. Even when proved wrong in detail, the general programme of Cartesian mechanism remained a guide to inquiry, and its general concepts also showed themselves admirably and fruitfully adaptable to the requirements of experimental results, as for example in physiology, in the theories of light of Hooke and Huygens, and in the later history of Descartes' *matiere subtile* or ether filling space (cf. above, p. 161).

The basis of Descartes' philosophy of nature was his division of created reality (i.e. as distinct from God) into two mutually exclusive and collectively exhaustive essences or 'simple natures,' extension and thought, and his conception of the method which was designed to give him certain knowledge of this reality. It is significant that Descartes should have resembled a medieval natural philosopher like Grosseteste or Roger Bacon in presenting his first published scientific results as examples of the application of a con-

ception of scientific method. The epoch-making volume of treatises published in 1637 had the full title: *Discours de la Méthode pour bien conduire sa raison, et chercher la vérité dans les sciences. Plus la Dioptrique, les Météores et la Géometrie, Qui sont des essais de cete Méthode.* The fact that two of these treatises should have dealt with optics and that his earliest cosmological essay should have had the sub-title *Traité de la Lumière* is also an indication of at least part of Descartes' intellectual ancestry. But before any of these works he had already, between 1619 and 1628, written his fullest treatise on method, his *Regulæ ad Directionem Ingenii*, published posthumously in 1701. Such an order of composition could scarcely show more strongly his confidently rationalist approach to science.

'By method,' Descartes wrote in Rule iv of the *Regulæ*, 'I mean a set of certain and easy rules such that anyone who obeys them exactly will first never take anything false for true and secondly, will advance by an orderly effort, step by step, without waste of mental effort, until he has achieved the knowledge of everything that does not surpass his capacity of understanding.' He went on in Rule v: 'The whole of method consists in the order and disposition of the objects to which the mind's attention must be turned, that we may discover some truth. And we will exactly observe this method, if we reduce involved and obscure propositions step by step to simpler ones, and then, from an intuition of the simplest ones of all, try to ascend through the same steps to the knowledge of all others.'

A distinction must be made between Descartes' method as applied to philosophy and as applied to science. So far as philosophy is concerned, the rules he gave for analysing the data of experience were to prepare the mind for an intuitive act, similar to that described by Aristotle at the end of the *Posterior Analytics,* by which the 'simple natures' were grasped. These were, for example, thought, extension, number, motion, existence, duration—self-evident 'clear and simple ideas' which could not be reduced to anything simpler and so had no logical definitions. The purpose of the rules was to choose and arrange the data for this act of intuition, and they included a form of induction

involving the principle of elimination. Descartes' philosophical aim was to reduce the 'involved and obscure propositions,' with which we began from experience, to propositions that were either self-evident (simple natures) or had been already shown to follow from self-evident propositions. Having done this, he would then be able to explain the whole of the data of experience by showing that they could be deduced from the discovered 'simple natures.' In his search for the 'simple natures' constituting the created world he held that he had been successful. The ultimate substance of everything was either *res extensa* or *res cogitans*. As he wrote in the *Principia Philosophiæ*, part 1, principle 53: 'Although any one attribute is sufficient to give us knowledge of substance, there is always one principal property of substance which constitutes its nature and essence, and on which all the others depend. Thus extension in length, breadth and depth constitutes the nature of bodily substance; and thought constitutes the nature of thinking substance. For all else that may be attributed to body presupposes extension, and is merely a mode of this extended thing; and in the same way everything that we find in mind is merely so many diverse forms of thinking. Thus, for example, we cannot conceive of shape except in an extended thing, nor of movement except in an extended space; and similarly imagination, feeling and will exist only in a thinking thing, and we cannot conceive of them without it. But we can, on the contrary, conceive of extension without shape and movement and of thinking thing without imagination or feeling, and similarly for the other attributes.'

In part 2, section 4 he asserted the identity of matter and extension even more emphatically, writing: 'The nature of matter, or of body in general, does not consist in its being a thing which is hard or heavy or coloured or which affects our senses in some other way, but only in its being a substance which is extended in length, breadth and depth. . . . Its nature consists simply in this, that it is a substance with extension.' Thus the secondary qualities were subjective; only extension and motion had any objective existence; and all the properties that we observed in

matter were due to the diversification of the original matter, under the influence of motion, into particles of different sizes and shapes and motions and their subsequent aggregation into bodies of various kinds. So anxious was Descartes to banish the substantial forms and all innate real qualities—'occult properties' that he excluded even the idea that bodies were naturally endowed with weight. It was for assuming gravity to be an innate quality, and for not attempting to explain it, that Descartes criticised Galileo to Mersenne (cf. above, p. 162). His own attempt to explain gravity was by the *matière subtile* or ether acting mechanically in the *plenum* of matter identified with extension. In this *plenum* all action was by contact; it excluded the possibility of a vacuum and was the basis of his theory of vortices; and it enabled him to exclude the 'occult force' of attraction at a distance.

When Descartes first discussed the application of his method to natural science he was as confident of success as he was in philosophy. The 'Universal Mathematics' adumbrated in the *Regulæ* was to repeat the structure of his philosophical system depending on the 'simple natures.' It was to embrace the whole physical world and to subordinate to itself all the particular sciences, and within this scheme science would discover the invariable cause, the invariable connection between the *datum* of experience and the *quæsitum* of theory. Here indeed would be a complete union of prediction and explanation, if only it could be proved.

Descartes' account of scientific method in the *Regulæ* was a variant on the familiar double procedure of analysis and synthesis or resolution and composition. The object of scientific inquiry was to reduce the complex problems, as presented by experience, which he described in somewhat Aristotelian language as 'composite *a parte rei,*' to specific constituent problems for quantitative solution, so that the complex situation could then be reconstituted theoretically and explained by deduction from the discovered elements and laws that produced it. The first stage of the analysis led to a classification of the data, and on the basis of these the investigator then set up hypothetical 'conjectures' of

the cause. These were required because the complexity of nature necessitated an indirect route to the truth, and the next stage was to deduce the empirical consequences that followed from them, and to eliminate false conjectures by applying the Baconian method of the *experimentum* or *instantia crucis*, using the methods of agreement, difference and concomitant variation. The 'composite' of theory showed the true cause when it corresponded perfectly with the 'composite' of things. So the theory explained the facts, and the facts proved the theory (cf. above, pp. 27, 208, below, p. 325). Descartes described this reciprocal movement as a 'demonstration,' writing in the *Discours*, part 6: 'If some of the matters of which I have spoken in the beginning of the *Dioptrics* and *Meteors* should offend at first sight, because I call them hypotheses and seem indifferent about giving proof of them, I request a patient and attentive reading of the whole, from which I hope those hesitating will derive satisfaction; for it appears to me that the reasonings are so mutually connected in those treatises, that, as the last are demonstrated by the first which are their causes, the first are in their turn demonstrated by the last which are their effects. Nor must it be imagined that I here commit the fallacy which logicians call a circle; for since experience renders the majority of the effects most certain, the causes from which I deduce them do not serve so much to establish their existence as to explain them; but on the contrary, the existence of the causes is established by the effects.'

An 'Augustinian-Platonist' in the same way as Grosseteste and Roger Bacon, just as they found certainty only in Divine illumination, so Descartes found it only in the belief that the most perfect of all Beings would not deceive him. Backed by that guarantee, he asserted, in a letter to Mersenne written on 27 May 1638, 'There are only two ways of refuting what I have written: one is to prove by some experiments or reasoning that the things I have assumed are false; and the other, that what I deduce from them cannot be deduced.' Unfortunately, as Newton delighted to show, on all too many occasions Descartes ex-

posed himself to refutation on just these grounds (cf. above, p. 161 *et seq.*).

Descartes' whole process of inquiry by means of conjectures presupposed the mechanical philosophy as the basis of the explanation, as distinct from the mere prediction or summary of the facts. For Descartes such explanations must always be the ultimate goal of scientific inquiry, because it was they that connected the particular phenomena of experience to the 'simple natures' that ultimately constituted the world and so provided the ultimate explanation of all phenomena. So by putting natural science into this philosophical framework Descartes made it necessary in some degree to answer the final question before asking the first one.

The same point of view appeared in his attitude to Harvey. In his description, in the *Traité de l'Homme*, of how the body could act according to purely mechanical laws, Descartes acclaimed Harvey's discovery of the circulation of the blood but refused to accept his account of the systole and diastole of the heart on the grounds that, even if Harvey's facts proved correct, he had not explained the *reason* for the heart's contraction. Descartes' own explanation of the heart-beat in fact rejected those of both Harvey and Galen alike and was a revival of Aristotle's conception of the heart as the centre of vital heat which caused the expulsion of the blood from the heart by making it boil and *expand* (see above, p. 238 *et seq.*). Later, in his *Description du Corps Humain* (1648; published 1672), Descartes admitted that '*une expérience fort apparente,*' such as one he suggested on the vivisection of a rabbit's heart, might confirm Harvey's account of the heart's motion, but he added: 'Nevertheless that only shows that the observations can often even lead us into being deceived, when we do not sufficiently examine all the causes which they could have.' Harvey's theory might be shown to agree with many of the phenomena, but 'that did not exclude the possibility that all the same effects might follow from another cause, namely from the dilatation of the blood which I have described. But in order to be able to decide which of these two causes is true, we must consider other ob-

servations which cannot agree with both of them.' The choice between the rival hypotheses must be made by an *experimentum crucis* which would falsify one of them.

The ultimate objective of Descartes' method, in science as in philosophy, was thus in the final analysis to display the connection by 'long chains of deduction' between the ultimate ontological reality, as discovered in the 'simple natures,' and the many particulars of experience. In this conception of an ultimately ontological goal of scientific discovery Descartes in fact agreed with Platonising mathematical physicists like Galileo and Kepler, who had introduced such empirical conviction into the identification of the substance of the real world with the mathematical entities contained in the theories used to predict the 'appearances.' It was not in this ultimate ontological goal, but in the smaller degree of empirical caution with which he moved towards it, that Descartes differed from these more empirical contemporaries.

It was in the extreme and systematic form given to it by Descartes, offering a comprehensive metaphysical and cosmological alternative to the Aristotelian philosophy, that the mechanical philosophy raised the philosophical problems that came to shape the character not only of the epistemology and metaphysics of the period but also of the philosophy of science. For example, the doctrine of the subjectivity of the 'secondary' qualities was taken up by Locke and incorporated into his new theory of knowledge, according to which the proper objects of our knowledge are not things in an external world but the data of experience received through the sense organs, and organised by the mind. This is not the place to discuss Locke's epistemology, but it is interesting that it should have been the 'restorer' of the mechanical philosophy himself, Robert Boyle, who pointed out that the primary qualities or geometrical concepts in terms of which mathematical physics organised and interpreted experience were no less mental than the secondary qualities, and that if either group had any claim to reality then both had equal claims. George Berkeley (1685–1753) was to make a similar criticism.

A whole range of problems was raised by Descartes' ab-

solute identification of matter with extension, aimed at the uncompromising exclusion from bodies of any innate properties whatsoever. In physics the difficulties this made in accounting for gravitation and in determining what was conserved in the conservation of motion became the main subjects of controversies between Huygens, Leibniz and the Newtonians. These are a good illustration of the metaphysical origin of many scientific concepts which were only later tailored to the requirements of quantitative precision (cf. above, p. 164). The total exclusion of the active principles in things corresponding to the scholastic 'natures' created a general difficulty for the whole doctrine of causation. Strictly speaking all 'secondary' causation (that is, causation apart from God's direct intervention) became impossible, as some of Descartes' followers pointed out. Some writers, for example Gassendi and Sir Kenelm Digby (1603–65), tried to deal with this general problem by returning to a form of atomism and, with some confusion, attributed efficient causality to the atoms themselves. A somewhat different solution to the whole problem of interaction was proposed by Leibniz with his theory of monads. These solutions came to have a considerable influence in biology, where the Cartesian doctrine of matter had caused great embarrassment by altogether excluding organisms. For example, when Maupertuis and Buffon (1707–88) tried to explain on mechanical principles such phenomena as the adaptation of the functions of the parts of living things to the needs of the whole and the teleological appearances of embryological development and of animal behaviour, they turned these particles in which causality was lodged into the '*molécules organisées*.' Maupertuis pointed out very clearly that mechanical concepts formulated to explain only a restricted range of inorganic phenomena must be expected to prove inadequate, when applied to other phenomena for which they were not designed. Since biological phenomena seemed to demand both active principles and teleology, his solution was to offer an explanation of them in terms of the antecedent movement of particles, whose behaviour anticipated the ends towards which they moved and the functions to be

served by the organs they formed. In developing this form of explanation Maupertuis came to put forward the first systematic theory of organic evolution, and to discuss for the first time in this context the production of order out of disorder by the operation of chance.

It was in the question of interaction between body and mind, between the absolutely distinct extended substance and thinking substance, that the Cartesian system brought out into the open the most intractable problem for the mechanical philosophy, and one that has profoundly affected the whole philosophy of nature that has been developed by scientists, especially physiologists, since the 17th century. For Aristotelian philosophy there was strictly speaking no mind-body problem, since the soul, the *animus* of the scholastics, which included the mind (cf. Vol. I, p. 163, note 11), was the 'form' of the human being, and determined the nature of the psycho-physical unity just as the form of an inanimate body determined its nature. The problem arose with the mechanistic conception of the body. Joseph Glanvill wrote rhetorically in *The Vanity of Dogmatizing* (1661): 'How the purer spirit is united to this Clod, is a knot too hard for fallen Humanity to unty.'

Descartes discussed the question principally in his *Traité de l'Homme, Les Passions de l'Âme*, and the *Principia Philosophiæ*. His procedure in formulating it was clear and intelligent. Accepting the distinction between mind (sensation, feeling, thinking) and matter (as conceived mechanically), he decided on philosophical grounds that there was interaction between them in the human body. The main philosophical grounds for this conclusion were that we could not deny the reality, for example, of the apparent power of the body to generate in us sensations and feelings, without regarding God as a deceiver, which would be incompatible with his perfection. Moreover there was no good reason to deny it. Consequently he looked for a connection between mind and body in an appropriate physiological mechanism, which he located in the pineal gland (cf. above, p. 240 *et seq.*).

Beginning with Gassendi, the critics of Descartes' theory

of interaction pointed out that any point of contact between the mutually exclusive extended unthinking substance and unextended thinking substance was ruled out by definition. This led to a re-examination of the terms of Descartes' formulation of the theory of interaction and to the development of three other solutions, parallelism, materialism, and phenomenalism. Between these four possibilities the problem has oscillated ever since.

Historically the first alternative to Cartesian interactionism was the form of parallelism known as 'occasionalism.' Developed principally by Geulincx (1625–69) and Nicolas Malebranche (1638–1715), this doctrine attributed all causal action immediately to God. When an event A seemed to produce another event B, they held that what really happened was that A furnished the occasion for God voluntarily to produce B. Thus although a physical event happening in the body might seem to produce a sensation in the mind, and an act of will might seem to produce a movement of the body, there was in fact no causal link between two such events except in God who produced them both. In his activities God usually followed fixed rules, so it was possible for natural philosophers to formulate general scientific laws. This was a position similar to that of Ockham (see above, p. 32).

The materialist solution of the mind-body problem was an attempt to reach the unity of theory at which science aims by showing that mental phenomena could be exhaustively derived from, or reduced to, the laws governing the behaviour of matter. The first modern author to put forward a materialist theory of this kind was Thomas Hobbes (1588–1679). It is natural that from the beginning materialism should have been associated with the motive of turning one half of the Cartesian duality into a system of anti-theological metaphysics, flying the banner of science. In the hands of the 'physiologists' of the French *Encyclopédie* like La Mettrie, D'Holbach, Condorcet and Cabanis, man became nothing but a machine; consciousness became a secretion of the brain just as bile was a secretion of the liver; and physical and physiological laws as they conceived them were taken as the norm of the laws

not only of mind but also of history and the historical progress of society. Directly descended from the Cartesian mechanical philosophy and Newtonian physics, these conceptions developed by the 18th-century French natural philosophers and sociologists, became the direct ancestors of the materialist doctrines associated with Charles Darwin's theory of evolution and its sociological extensions in the 19th-century doctrine of progress.

The phenomenalist, or idealist, solution aimed at getting rid of the Cartesian dualism by taking as the primary objects of knowledge not things in an external world known by means of sensation, but the data of sensation themselves. The physical world was then regarded as a mental construction from these data, existing only in a mind, although, as Berkeley argued, the only mind in which it could properly be said to exist was God's mind. It is characteristic of this doctrine that, in opposition to materialism, it was widely associated with the motive of saving theology from the conclusions that were being drawn from science and from the mechanical philosophy by writers motivated in the opposite direction.

Indeed the whole development of philosophy in relation to science, and of the philosophy of science, since the 17th century is properly intelligible only within the wider context of the beliefs, especially the theological beliefs, of the period. Undoubtedly the dualism of the mechanical philosophy led to a feeling of bleak isolation of the human spirit, knowing beauty, conscience and the simple pleasures of the secondary qualities, in an inhuman infinity of matter-in-motion. 'Thus is Man that great and true Amphibium,' Sir Thomas Browne pointed the contrast in *Religio Medici* (1643), in his vivid baroque, 'whose nature is disposed to live, not only like other creatures in divers elements but in divided and distinguished worlds.' This reflects an effect on the sensibility that certainly forms part of the so-called 'crisis of conscience' to which the Scientific Revolution gave rise. But there were also specific theological doctrines whose practical influence on contemporary philosophy was probably much more important. For example Descartes, acting with unquestionable sincerity, kept a

sharp eye on the doctrine of transubstantiation when developing his theory of matter and of material change. When he heard of Galileo's condemnation on the strength of certain Scriptural texts, he was prepared, with perhaps less unquestionable sincerity, to change his whole philosophy (cf. above, p. 216 *et seq.*).

Considerable light is thrown on the position in which Galileo and Descartes found themselves in relation to contemporary theology by recalling the moves that followed the introduction of Aristotelian philosophy into the West in the 13th century (cf. Vol. I, pp. 55–64; above, 34–35). The Aristotelian system came into circulation accompanied by the Averroïstic doctrines that the universe was a necessarily determined emanation from God's reason, instead of a free creation of his will as Christian theology taught; that the ultimate rational causes of things in God's mind could be discovered by the human reason; and that Aristotle had in fact discovered those causes, so that the universe must necessarily be constituted as he had described it, and could not be otherwise. By means of the Christian doctrines of the inscrutability and absolute omnipotence of God, the 13th-century theologians and philosophers liberated rational and empirical inquiry into the laws that nature in fact exhibits from this absolute subjection to a metaphysical system. The price of this liberation, however, was a much less exacting subjection to the revealed Christian doctrines, and especially to that of the truth of the word (literal or interpreted) of Scripture. Galileo no less than Oresme was prepared willingly to pay this price, though not in the currency pressed into his hand. What he rejected was in fact the currency of Ockham, who, in his anxiety to save the content of revelation from any possibility of threat from the side of reason, had made radical further use of the doctrine of God's absolute omnipotence to destroy the rational content of science altogether. The observed regularities of the world became mere regularities of fact, and the laws expressing them became at their strongest mere possibilities, at their weakest simply conventional devices for correlation and calculation.

The currency that Galileo flung aside when it was of-

fered to him by Bellarmine and by Pope Urban VIII, Descartes was quick to make his own. At the outset of his philosophical and scientific inquiries Descartes had written with the greatest confidence of being able to discover true and ultimate explanations. But after 1633 he became the *'philosophe au masque.'* He withdrew *Le Monde,* and in the revised version of his system published in *Principia Philosophiæ* in 1644 he made his famous declaration of scientific theories as mere fictions. 'I want what I have written to be taken simply as an hypothesis, which is perhaps far removed from the truth; but yet that having been done, I believe that it will have been well done if everything deduced from it agrees completely with the observations. For if that happens it will be no less useful in practice than if it were true, because we can use it in just the same way to set out the natural causes to produce the effects we want.' (Part 3, section 44.) He continued (in section 45): 'I shall assume here some things which I believe to be false.' For example, he believed that, as the Christian religion required, God had created the world complete at the beginning, and with God's omnipotence this was reasonable. But we could sometimes understand the general natures of things better by supposing hypotheses which we did not believe to be literally true, for example that all organisms came from seeds, 'although we know that they have not been produced in this way, if we are to describe the world only as it is, or rather as we believe that it was created.' He concluded, in section 47: 'Their falsity does not prevent that which may be deduced from them from being true.'

The policy indicated in these passages, the policy of Ockham, of Osiander, of Bellarmine, was aimed primarily not at interpreting the theoretical formulations of science but at tolerance between them and Christian theology. It was aimed at showing not only that the development of an anti-theological metaphysics was not a necessary consequence of the mechanical philosophy of science, but that science was in fact unable to yield any metaphysics at all. Adopted out of prudence, it is oddly placed in Descartes' philosophical outlook as a whole. It provided an escape

clause allowing the practice of science to go on even in the face of theological propositions it might seem to contradict.

Many other aspects of 17th-century thought reflect the same tendency to avoid difficulty by separating scientific problems from theological and metaphysical entanglements as completely as possible. An example of this can be seen in occasionalism, for since God's will is inscrutable the occasionalist is left in fact only with observation and correlation as the proper objects of scientific inquiry.

It became a characteristic of many scientists of the period, of Mersenne, Pascal, Roberval, Mariotte, to refuse to discuss 'causes' in their physical inquiries; and likewise the Royal Society, consciously avoiding contentious subjects, became heavily experimental. The same policy of separating natural science from questions of ultimate causes was expressed by Boyle when he wrote in *The Excellency and Grounds of the Mechanical Hypothesis* (*Works*, abridged by Peter Shaw, 1725, vol. i, p. 187): "The philosophy I plead for, reaches but to things purely corporeal; and distinguishing between the first origin of things, and the subsequent course of nature, teaches that God . . . establish'd those rules of motion, and that order amongst things corporeal, which we call the laws of nature. Thus, the universe being once fram'd by God, and the laws of motion settled, and all upheld by his perpetual concourse and general providence . . . the phenomena of the world are physically produced by the mechanical properties of the parts of matter.'

As events turned out, none of these moves to avoid theological trouble succeeded in their objectives. The advance of science did in fact give rise to materialist metaphysics, naïve certainly but to become nevertheless influential in the 18th and 19th centuries, and by definition anti-theological. The God of the scientists, of Boyle, the 'intelligent and powerful being' praised by Newton in the *Principia*, when taken over by the 18th-century Deists, no longer gave any primacy or uniqueness to Christianity among the religions. Most corrosive of all, the 'fictionalist' or 'conven-

tionalist' policy adopted by Descartes and pressed forward
by Berkeley, became in the hands of secular philosophers
like David Hume (1711–76), and of Immanuel Kant
(1724–1804), the source of a doctrine that was anti-
rational and anti-theological alike. Applied universally, as
it inevitably was, it ceased to be a defence of theology
against science and became a threat to all knowledge,
whether rational or revealed. The way was open to the ex-
plicitly anti-theological and anti-metaphysical positivism
of Auguste Comte (1798–1857) and John Stuart Mill
(1806–73), and to the agnosticism of T. H. Huxley, which
became so characteristic a part of the philosophical am-
bience of science in the 19th century. This was a conse-
quence of the influence of their intellectual careers in
which neither Galileo nor Descartes would have taken any
pleasure, yet in some degree both foresaw it.

It would be misleading to leave the impression that all
discussion of the philosophy of science in the 17th and
18th centuries was directed only at taking an attitude to
theology. Dropping the crudely theological objective of
Bellarmine and Descartes, the problem for philosophers
became the relation of scientific knowledge to the possi-
bilities of knowledge in general. From the time of Des-
cartes the justification of the assumptions, procedures and
conclusions of the new science became an essential part of
the general problem of knowledge, which included the
questions both of finding explanations (as distinct from
mere predictions) in science and of the possibility of ra-
tional theology. All the great philosophers following Des-
cartes, especially Leibniz, Berkeley, Kant, and Mill, con-
tributed profoundly to the philosophy of science and were
themselves profoundly influenced by their analyses of sci-
entific thought.

No less important, both for the general philosophical
atmosphere generated by science and for the philosophy
of science, were the discussions of problems in this field
by scientists themselves. Although these can be properly un-
derstood only within the wider philosophical context, they
had in fact a distinctive objective. Where philosophers
were primarily interested in science in relation to the gen-

eral problem of knowledge, scientists usually became interested in the philosophy of science primarily in relation to specific problems encountered in the course of their scientific work. Many of these were not essential to a purely scientific solution. For example it is not necessary to discuss the mind-body problem in order to investigate the physiology of the brain and sense-organs, or to discuss the admissibility of action at a distance in order to investigate the laws of planetary motion. Nevertheless it was necessary that investigators looking for explanations from science should discuss such problems. No doubt because of their different objectives, the 20th-century dichotomy between the philosophy of science of scientists and that of philosophers can be seen in embryo even in the 17th century. Each tending more and more to ignore the writings of the other, the division solidified in practically all European educational systems in the 19th century, to the increasing disadvantage of both sides.

The discussions of the philosophy of science by scientists that most profoundly influenced the development of scientific thought in the 17th century all concerned the relationship between specific theories formulated for the purpose of predicting particular phenomena, and the mechanical philosophy of nature in terms of which it was assumed that all explanations in physics must be given. In fact the problem was similar to that existing between the predictive theories of the 13th and 14th centuries and the Aristotelian philosophy of nature. By the time the Royal Society had received its first charter in 1662 and the Académie des Sciences had been established in 1666, the attitudes to the problem had tended to polarise around the two dominant philosophies of science of the period, the empiricism and experimentalism inspired by Bacon and Galileo with its inveterate dislike of systems, and the Cartesian rationalism with its unifying conception of universal principles applying to every aspect of the physical world. The former was favoured by the majority of the English and the latter had its strongest supporters in France and Holland, but in fact no natural philosopher of the period escaped the influence of both. It was from the

English experimental school, especially from Boyle and Newton, that the philosophy of science of the scientists as distinct from the philosophers received its most characteristic expression. Boyle and Newton were as convinced as Galileo that science discovered in its theories genuine knowledge about a real and objective natural world. But while the discovery of explanations and real causes remained their ultimate goal, they pursued a tough-minded policy of distinguishing sharply between experimentally established laws that gave accurate predictions, and the assumptions of the accepted philosophy of nature. Details of this last, especially those added speculatively by Descartes, they were always prepared to shelve. Thus they objected equally to the idea that scientific theories were mere fictions or calculating devices, and to the new scholasticism into which Descartes' lesser followers had crystallised his mechanical system. Their real contribution to contemporary and, indeed, to all succeeding philosophy of science was their systematic use of the experimental principle of verification and falsification to distinguish clearly between the different kinds of statements involved in a scientific system. The attitude taken up by this experimental school was well characterised by William Wotton in 1694 in his *Reflections upon Ancient and Modern Learning*: 'And therefore,' he wrote in chapter 20, 'that it may not be thought that I mistake every plausible Notion of a Witty Philosopher for a new Discovery of Nature, I must desire that my former Distinction between *Hypotheses* and *Theories* may be remembered. I do not here reckon the several *Hypotheses* of *Des Cartes, Gassendi,* or *Hobbes,* as Acquisitions to real Knowledge, since they may only be Chimæras, and amusing Notions, fit to entertain working Heads. I only alledge such Doctrines as are raised upon faithful Experiments, and nice Observations; and such Consequences as are the immediate Results of, and manifest Corollaries drawn from these Experiments and Observations: Which is what is commonly meant by *Theories*.'

It was Newton, becoming the acknowledged master of the experimental philosophy, who achieved the clearest

appreciation of the relation between the empirical elements in a scientific system and the hypothetical elements derived from a philosophy of nature. Newton wrote no systematic philosophy of science, but like Galileo he was forced into discussions of scientific method by the controversies to which both his theory of colour and his theory of gravitation gave rise. Both were said by Cartesian critics, and especially by Huygens and Leibniz, to be descriptive and predictive but not explanatory. Presented in the context of controversy, and always in relation to specific problems, his statements have led to considerable misunderstanding. But they clearly indicate a consistent policy throughout. Forced into discussion by Huygens' criticism of his 'New Theory about Light and Colours,' published in the *Philosophical Transactions of the Royal Society* in 1671–72, it was in the subsequent controversy that Newton first took up his characteristic position. He pointed out first that his inquiry into the phenomenal laws was independent of any inquiry into the causes or mechanical processes producing them; secondly that it was only after the phenomenal laws had been established experimentally as the data to be explained that the inquiry for the explanation could begin with hope of success; and thirdly that no experimentally established law could be refuted because it was contradicted by an hypothesis about the causes of the phenomena. As he wrote on 2 June 1672 to Henry Oldenburg, the secretary of the Royal Society, in a letter printed in Samuel Horsley's edition of Newton's *Opera* (1782, vol. 4, pp. 314–15): 'For the best and safest method of philosophizing seems to be, first diligently to investigate the properties of things and establish them by experiment, and then to seek hypotheses to explain them. For hypotheses ought to be fitted merely to explain the properties of things and not attempt to predetermine them except in so far as they can be an aid to experiments. If any one offers conjectures about the truth of things from the mere possibility of hypotheses, I do not see how any thing can be determined in any science; for it is always possible to contrive hypotheses, one after another, which are found rich in new tribulations. Wherefore I judged that one should

abstain from considering hypotheses as from a fallacious argument, and that the force of their opposition must be removed, that one may arrive at a maturer and more general explanation.' He was to make these points again, in defence of his theory of gravitation, in query 31 of the *Opticks* (1706) and in the Rules of Reasoning in Philosophy, especially Rule iv (1726), at the beginning of the third book of the *Principia*.

From this eminently reasonable position Newton brought clarity into the whole subject of scientific method and logic, and established a policy that was both critical and fruitful for dealing with the relation between the data and phenomenal laws on the one hand, and hypotheses about causes on the other. By means of this policy he showed how mechanical hypotheses could be a fruitful guide to research without becoming misleading. Indeed, possibly because he was not deceived about their hypothetical status, where others would propose one explanation and defend it against all objections, his fertile mind would suggest a whole range of hypotheses, for example of the ether as an explanation of the phenomena of light, gravitation, cohesion, electric and magnetic attraction. Far from excluding from the competence of science the discovery of the real processes in nature causing the phenomenal laws, Newton in fact took them so seriously as the ultimate objective of scientific inquiry that he insisted that the investigation of causes must be conducted as rigorously as that of the laws themselves. "There are therefore Agents in Nature able to make the Particles of Bodies stick together by very strong Attractions,' he exclaimed in query 31 of the *Opticks,* 'And it is the Business of experimental Philosophy to find them out.' The famous aphorism, *hypotheses non fingo,* in the General Scholium at the end of Book 3 in the second edition of the *Principia* (1713), was directed, as Koyré has pointed out, not against hypotheses about real causes, but against Cartesian fictions and fictionalism. Indeed it is likely that he chose the title *Principia Mathematica* in order to give direct point to his polemic against Descartes' *Principia Philosophiæ*. Thus Newton reversed Descartes' rebuke to Galileo for not pro-

viding explanations, and did so by Galileo's own methods of science, which he brought to completion.

Newton certainly did not regard scientific laws as mere predicting devices. They were written in the phenomena, though they were not open to direct inspection and had to be discovered or 'inferred' or 'deduced' from the phenomena by appropriate mathematical and experimental analysis. In the sense that he was searching for true explanations, Newton had the same objective as Aristotle and all his intellectual descendants. But the Aristotelian 'natures' offered explanations divorced from predictive laws. It was this divorce that had occasioned the whole discussion between prediction and explanation since the 13th century and had led to the replacement of Aristotelian physics by the mathematical and mechanical philosophy of nature. As Newton wrote of the Aristotelian 'natures' in query 31 of the *Opticks*, echoing Galileo:

> Such occult Qualities put a stop to the Improvement of natural Philosophy, and therefore of late Years have been rejected. To tell us that every Species of Things is endow'd with an occult specified Quality by which it acts and produces its manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phænomena, and afterward to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in Philosophy, though the Causes of those Principles were not yet discover'd: And therefore I scruple not to propose the Principles of Motion above-mention'd, they being of very general Extent, and leave their Causes to be found out.

By applying the same rigorous quantitative methods to hypotheses about causes as to laws, Newton wanted to point the way towards the goal of the whole experimental school of natural philosophy: the union of explanatory theory and predictive laws in a single theoretical system. Thus, having solved, by means of his laws of motion and of gravitation, the problem of the dynamics of macroscopic bodies on earth and in the heavens, he wrote in the preface

to the first edition of the *Principia*: 'I wish we could derive the rest of the phenomena of Nature by the same kind of reasoning from mechanical principles, for I am induced by many reasons to suspect that they may all depend upon certain forces by which the particles of bodies, by some causes hitherto unknown, are either mutually impelled towards one another, and cohere in regular figures, or are repelled and recede from one another. These forces being unknown, philosophers have hitherto attempted the search of Nature in vain; but I hope the principles here laid down will afford some light either to this or some truer method of philosophy.'

Two further passages indicate the continuity of the logical structure of his science with the long tradition stretching back through Galileo and the medieval writers on the 'resolutive-compositive' method to the Greek geometers (cf. above, p. 13). In query 31 of the *Opticks* he wrote:

As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy.[24] And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phænomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur. By this way of Analysis we may proceed from Compounds to Ingredients, and

[24] That is, hypotheses in the sense of explicit fictions.

from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Analysis: And the Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phænomena proceeding from them, and proving the Explanations.

Replying in 1712 to Roger Cotes, who was seeing the second edition of the *Principia* (1713) through the press, Newton wrote to clarify further his conception of the distinctions to be made between the different propositions of a scientific system. His purpose was to explain the phrase *hypotheses non fingo* in the General Scholium. He wrote: '. . . as in Geometry the word Hypothesis is not taken in so large a sense as to include the Axioms and Postulates, so in Experimental Philosophy it is not to be taken in so large a sense as to include the first Principles or Axioms which I call the laws of motion. These Principles are deduced from Phænomena and made general by Induction: which is the highest evidence that a Proposition can have in this philosophy. And the word Hypothesis is here used by me to signify only such a Proposition as is not a Phænomenon nor deduced from any Phænomena but assumed or supposed without any experimental proof.'

In one case Newton seems to have meant that laws (or 'principles') were 'deduced from phenomena' in the strict and literal sense, for he showed that just as Kepler's planetary laws could be deduced from the laws of motion and the inverse-square law of gravitation, so the last could be deduced from Kepler's Third Law, describing the phenomena. What he had done in fact was to demonstrate a reciprocal implication between a more and a less general law; his other statements show that he recognised clearly that this does not apply to the relationship between a law and the phenomenal data. In the search for certainty in science, the reciprocal relationship represented an ideal derived from mathematics (cf. above, pp. 27, 193, 199). Showing clearly the 'Euclidean' conception of the structure

of theoretical science established by the long tradition which he had inherited, the purpose of Newton's distinctions was to state explicitly the extent to which the first principles of a science and of an explanation could be said to have been verified. In the controversies on this question into which his explanations of colour and of planetary motion had drawn him, his policy was to reject, on the one hand, hypotheses proposed as explicit fictions and, on the other, the use of hypotheses of any kind as objections to experimentally established laws, against which the only objections could be contrary experimental evidence or proof of logical inconsistency. So he concluded finally in *Rule* iv in book 3 of the third edition of the *Principia* (1726): 'In experimental philosophy we are to look upon propositions inferred by general induction (*per inductionem collectæ*) as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions. This rule we must follow, that the argument of induction may not be evaded by hypotheses.'

A further well-known passage, from the preface to Huygens' *Traité de la Lumière* (1690), shows how far the method of reasoning in the new physics of the 17th century had moved from the Greek conception of geometrical demonstration. Instead of the justification of conclusions by showing them to be the necessary consequences deduced from first principles accepted as axiomatic, attention is now transferred to justification of the theoretical principles themselves by their observable consequences. It is asserted that the test by consequences achieves not certainty but only probability. The probability of a theory being true is said to increase with the number and range of confirmations, especially in predicting new phenomena. And it is claimed that this method enables us to discover the causes of events. 'There is to be found here,' Huygens wrote, 'a kind of demonstration that does not produce so great a certainty as that of geometry, and is indeed very different from that used by geometers, since they prove their propositions by certain and incontestable principles, whereas

here principles are tested by the consequences derived from them. The nature of the subject permits no other treatment. Nevertheless it is possible to reach in this way a degree of probability that is often scarcely less than complete certainty. This happens when the consequences of our assumed principles agree perfectly with the observed phenomena, and especially when such confirmations are numerous, but above all when we can imagine and foresee new phenomena which should follow from the hypotheses we employ and then find our expectations fulfilled. If in the following treatise all these evidences of probability are to be found together, as I think they are, this ought to be a very strong confirmation of the success of my inquiry, and it is scarcely possible that things should not be almost exactly as I have represented them. I venture to hope, therefore, that those who enjoy finding out the causes of things and can appreciate the wonders of light will be interested in these various speculations about it.'

For two centuries it was widely held by scientists that Newton had himself provided just such a union between prediction and explanation as all had been searching for, but already among Newton's earliest critics there were philosophers who did not share his optimism that science could discover 'causes' at all. Newton himself had stressed the sharp empirical distinction that *in fact* existed between knowledge of laws and of causes as envisaged by the current philosophy of nature. Reviving the conclusion reached by the scholastic logicians from Grosseteste to Nifo and Zabarella, that the data of observation cannot uniquely determine the theory that explains them, some 18th-century philosophers began to see the results of scientific inquiry less as discoveries about nature than as products of the methods of thought used.

The most acute of the contemporary critics of the Newtonian system was Berkeley, who in his *De Motu* (1721) anticipated much of Mach's famous analysis of Newton's basic assumptions. Developing arguments similar to those used by the medieval logicians, Berkeley came to the conclusion that neither the Newtonian system nor any other scientific theory could give an account of 'the nature of

things' or establish the causes of phenomena. Such a physi-
cal system was a 'mathematical hypothesis'; it established
simply the 'rules' by which phenomena were found to be
connected, and by means of which they could be predicted.
Berkeley claimed that there was no justification for New-
ton's conceptions of absolute space and time and that all
motion was relative.

Hume, the 18th-century Ockham, went even further
than Berkeley in claiming that science was irrational and
that explanation was strictly speaking impossible. Since the
empirical data did not carry their own explanation or give
grounds for belief in causality, and since he could see no
other grounds, he concluded that there was nothing objec-
tive in causal necessity beyond regular concomitance and
sequence. 'In a word, then,' he declared in section 4 of his
Inquiry Concerning Human Understanding, 'every effect is
a distinct event from its cause. It could not, therefore, be
discovered in the cause; and the first invention or concep-
tion of it, *a priori*, must be entirely arbitrary.'

An analogous 'nominalist' view of biological categories
above that of species was developed by Buffon (1707–88)
and other biologists in their critique of Linnæus' 'realist'
system of classification. Buffon declared that nature con-
tained only individuals; that the species, defined as the
succession of individuals capable of interbreeding, was a
real category; but that the 'family' and the higher categories
were mere names.

Awakened by Hume's critique, yet firmly believing in the
truth of the Newtonian system, to the extension of which
in fact he made a contribution as a physicist, Kant found
himself able to admit Newtonian science as true only at
the price of denying that it had discovered a real world of
nature behind the world of appearance. Similarly he found
himself obliged to deny the possibility of rational knowl-
edge of God, in whom he also firmly believed. Kant could
admit Newtonian science as a true science of nature pre-
cisely because he came to regard nature itself as the world
of phenomena, the world as it appeared to our assimilating
minds, and because he came to regard scientific theories as
products of the methods of organising experience, meth-

ods dictated by the structure of our minds. Because of that structure Kant believed that the scientist approached nature with certain necessary principles in mind, of which Euclid's propositions were explicit formulations, and that he necessarily presupposed these principles in all his knowledge and in all the theories with which he attempted to organise his experience. It was this view of science, the reflection of a philosophical situation produced by the success of the scientific revolution itself, as seen by a mind acutely aware of the processes of theoretical construction, that Kant described in his brilliant preface to the second edition of the *Critique of Pure Reason* (1781):

When Galileo let balls of a particular weight, which he had determined himself, roll down an inclined plane, or Torricelli made the air carry a weight, which he had previously determined to be equal to that of a definite volume of water; or when, in later times, Stahl changed metal into calx, and calx again into metal, by withdrawing and restoring something; a new light flashed on all students of nature. They comprehended that reason has insight into that only, which she herself produces on her own plan, and that she must move forward with the principles of her judgements, according to fixed law, and compel nature to answer her questions, but not let herself be led by nature, as it were in leading strings, because otherwise accidental observations, made on no previously fixed plan, will never converge towards a necessary law, which is the only thing that reason seeks and requires. Reason, holding in one hand its principles, according to which concordant phenomena alone can be admitted as laws of nature, and in the other hand the experiment, which it has devised according to those principles, must approach nature, in order to be taught by it: but not in the character of a pupil, who agrees to everything the master likes, but as an appointed judge, who compels the witnesses to answer the questions which he himself proposes. Therefore even the science of physics entirely owes the beneficial revolution in its character to the happy thought, that we ought to seek in

nature (and not import into it by means of fiction) what-
ever reason must learn from nature, and could not know
by itself, and that we must do this in accordance with
what reason itself has originally placed into nature. Thus
only has the study of nature entered on the secure
method of a science, after having for many centuries
done nothing but grope in the dark.[25]

All the subsequent philosophies of science that have de-
veloped in the 19th and 20th centuries have taken their
shape in one way or another from the doctrines developed
from Francis Bacon, Galileo and Descartes to Kant. It was
for example an easy step from Kant's view that theories
are read not in but *into* nature, to Auguste Comte's as-
sertion that the real goal of science was and always had
been not knowledge at all, but only power (cf. above,
p. 315). Seizing only one half of Bacon's Great Instaura-
tion, Comte declared in his *Cours de Philosophie Positive*
(1830), Première Leçon, that the object of science was
'*savoir, pour prévoir,*' in effect prediction to give control.
This needed only knowledge of empirical sequences, and to
ask for knowledge of the nature of things beyond this was

[25] This passage has a suggestive position in the parallel develop-
ment of conceptions of nature and of thought. At least since
Francis Bacon, philosophers and scientists had been reducing
nature to matter in motion and, somewhat later, thought to the
association of impressions and ideas. The behaviour of both bodies
and minds was determined by external events. Kant's pre-critical
and critical writings both indicate a concern with the mechanism-
organism problem. In the 'Critique of the Teleological Judgement'
(Part 2 of the *Critique of Judgement,* 1790), a brilliant contribu-
tion to the philosophy of biology, he made a point of the impos-
sibility in principle of explaining the facts of organic unity in
mechanistic terms, even though all the parts of the unity could be
analysed mechanistically. Thus he concluded that a living organism
was not a mere aggregate of unrelated mechanistic constituents,
but a functionally related system of parts bound together by a
principle of unity. Analogously, in the *Critique of Pure Reason,* he
gave to the mind a principle whereby it determined the connec-
tions of impressions and ideas according to its own plan. In both
cases the emphasis is placed on the actively controlling role of the
intrinsic principle, and in this Kant reintroduces something like
Aristotle's matter and form in opposition to the mechanical philos-
ophy of the 17th century.

not only useless but was to ask for something unattainable. It was to provide sure means of establishing such empirical connections that Comte's friend John Stuart Mill developed his own systematic account of scientific method. On the opposite side, Kant's account of scientific inquiry not as a mere dissection of nature, but as a process of active questioning in the light of preconceived principles, was used by William Whewell in his emphasis, in opposition to Comte and Mill, on the role of 'ideas' and hypotheses in scientific inquiry. Harking back to the 'argomento *ex suppositione*' and the 'resolutive-compositive' method of Galileo, the same point has been made by Mill's recent critics in stressing the 'hypothetico-deductive' structure of science. Twentieth-century 'conventionalism,' immediately the result largely of internal developments of physics which led to the abandonment of some of Newton's basic principles and the use of non-Euclidean geometries to 'save the appearances,' is likewise both an advance on the position reached by Kant and a return to an earlier position. Physics itself having disposed at least of the necessity of assuming Euclid's principles, the conception has grown, especially under the influence of Mach, Henri Poincaré and Duhem, that any theoretical system can be used to correlate experience, provided it passes the tests of logical coherence and experimental verification. Saluting the attempts made from Simplicius to Bellarmine to make sense of the state of astronomical theory before Kepler, the attempts of this school to deal with an analogous modern problem have made the choice of a system, apart from these tests, simply a matter of convenience and convention.

At the beginning of the European philosophical adventure, the search for the rational intelligibility of the world as we experience it, Hesiod's Muses announced darkly: 'We know how to tell many fictions that wear the guise of truth; but we know also how to declare the truth, when we will.' Lacking the gift of oracular understanding, the men who have in fact conducted the adventure since Greek times have themselves been able to make this philosophical distinction only by searching not only for the truth but also for principles for distinguishing truth from falsehood. Ever

since the Greeks took the decisive step in cosmology of looking for explanations deductively connected with the means of prediction, the step by which they established the European scientific tradition as distinct for example from Babylonian astronomy in which there was a total logical disjunction between the highly-developed technological predictions and the myths that did service for explanations, the problem of finding criteria for distinguishing true explanations from false has been a pre-eminent question in the growth of science. Seeking as they did knowledge as well as utility, the Greeks established European science as a philosophical activity different both from Eastern technology which largely knew no science and from Western technology which is science applied.

Inevitably in such an enterprise conceptions of scientific truth have themselves undergone development under the impact both of the internal problems of science and of philosophical criticism. But through the diversity of such conceptions and of actual scientific performance, from Plato's time to the present, the philosophical policy of science has remained consistently the same. Of this there could be no more telling illustration than that provided by the period reviewed in the foregoing pages. Apparently so full of metaphysical and theological distractions, even these were turned to good account, first in the conception of a system of rational explanations as such, and eventually in the great theoretical formulations of the period of Kepler and Galileo. The creative processes of original discovery and invention, always mysterious, are as little open to direct inspection as the laws of nature themselves. It is part of the philosophical enlightenment provided by the history of science to discover that the thought of great innovators whose effectiveness we admire was organised on a pattern in many ways so utterly different from our own, that they accepted a complex of non-empirical conceptions and 'regulative beliefs' that, alien though they are to us, nevertheless gave construction and form to theories of the greatest predictive and explanatory power. But it is a further part of enlightenment to discover that in spite of immediate appearances the policy for dealing with such a

pattern of thought, the criteria of verification and the objective towards which they are applied, has preserved its essential continuity throughout the whole European tradition.

Putting forward theories as true, but always submitting them to the experimental test, the intuition that has governed the scientific tradition has been characterised by Pascal in his *Pensées* (395): '*Nous avons une impuissance de prouver, invincible à tout le dogmatisme. Nous avons une idée de la vérité, invincible à tout le pyrrhonisme.*' Balanced between intuition and reason, between imagination and experiment, philosophical opinion in relation to science has oscillated between the extremes of scepticism and rationalism according to whether claims to have discovered ultimate reality, putting a stop to all further inquiry, or claims that no rational knowledge is possible at all, reducing science to an irrational technology, have presented most danger to the hopes of the moment. 'For who will prescribe bounds to man's intelligence and invention?' asked Galileo, the scientific realist, in 1615. 'Who will assert that all that is sensible and knowable in the world is already discovered and known?' It is through the development of this pragmatic policy of taking each case separately on its merits, of refusing to be bound by its own constructions, that the history of the Scientific Revolution throws its most significant light, not only on the nature of science itself, but also on all those other aspects of modern European thought that have arisen from an attitude taken to its methods and conclusions.

PLATE VIII. The lower part of the page reads as follows:
   'My first observation & others following of the new found
planets about Jupiter.
1610 Syon.
   1. Octob. 17. ☿ [Mercury]. ho. 12ª. 1ª. 2ª. I saw but
      one, & that above.
      Blackfriers, London.
   2. November. 16. ♀ [Venus]. ho. 9ª. 10ª. I saw one fayre
      9' or 10' above, and sometimes I thought I saw an
      other very small betwixt them, 3' or 4' à ♃ [Jupiter].
      London.
   3. November. 19. ☽ [moon]. ho. 9ª. one under fayre.
   4. Syon. Novemb. 28. ☿ . ho. 9ª. one under. fayre.
   5. November. 30. ♀ . ho. 9ª one above. fayre.
   6. Decemb. 4. ☿ . ho. 9ª. one under. fayre.
   7. Decemb. 7. ho. 9ª. 9ª½. I saw but one, & above.
   8. Mane. ho. 17°. Two seen on the west side, a little
      under. Sir W. Lower also saw them here. The nerest
      fayrest. The farther not well seen within the reach
      of my instrument of 20/1 of 14' dyameter.'
PLATE XII. 'The heart drives out the blood in its restrain-
ing . . . This thing was ordained by Nature in order that,
when the right ventricle begins to shut, the escape of the
blood out of its big capacity should not suddenly cease,
because some of that blood had to be given to the lung;
and none of it would have been given, if the valves had
prohibited the exit; (but this ventricle shut, when the lung
had received its quantity of blood, and when the right ven-
tricle could press through the pores of the medium wall
into the left ventricle); and at this time the right auricle
made itself the depository of the superabundance of the
blood which it advances to the lung that suddenly renders
it to the opening of this right ventricle, restoring itself

INDEX